MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXXVII. DECEMBER 8, 1876. No. 2.

WILLIAM HUGGINS, ESQ., F.R.S., President, in the Chair.

Capt. James Waterhouse, Surveyor General's Office, Calcutta,

was balloted for and duly elected a Fellow of the Society.

Note on Prof. Langley's Paper on the Direct Effect of Sun-spots on Terrestrial Climates. By W. Mattieu Williams, Esq.

Prof. Langley determines quantitatively the effects respectively produced by the radiations from the solar spots, penumbra, and photosphere upon the face of a thermopile, and infers that these effects measure their relative influence on terrestrial climate.

In thus assuming that the heat communicated to the thermopile measures the solar contribution to terrestrial climate, Prof. Langley omits an important factor, viz. the amount of heat absorbed in traversing the Earth's atmosphere; and in measuring the relative efficiency of the spots, penumbra, and photosphere, he has not taken into account the variations of diathermancy of the intervening atmospheric matter, which are due to the variations in the source of heat.

Speaking generally, it may be affirmed that the radiations of obscure heat are more largely absorbed by the gases and vapours of our atmosphere than those of luminous heat, and the great differences in the mere luminosity of the spots, penumbra, and photosphere justify the assumption that the radiations of a sun-spot will (to use the expressive simile of Tyndall) lose far more by atmospheric sifting than will those from the photosphere.

But the spot areas will be none the less effective on terrestrial climate on that account. A given amount of heat arrested by the Earth's atmosphere will have even greater climatic efficiency than if received upon its solid surface, inasmuch as the gases are worse radiators than the rocks, and will therefore, *cæteris paribus*, retain a larger proportion of the heat they receive.

I have long ago endeavoured to show * that the depth of the photosphere, from the solar surface inwards, is limited by dissociation; that the materials of the Sun within the photosphere exist in a dissociated, elementary condition; that at the photosphere they are, for the most part, combined. This view has since been adopted by many eminent solar physicists, and if correct, demands a much higher temperature within the depths revealed by that withdrawal of the photospheric veil which constitutes a sun-spot.

If I am right in this, and also in supposing the spot-radiations to be so much more abundantly absorbed than those of the photosphere, that in spite of the higher temperature of the spots, the *surface* of the earth receives from them the lower degree of heat measured by Prof. Langley, another interesting consequence must follow. The excess of spot-heat directly absorbed by the atmosphere, and mainly by the water dissolved or suspended in its upper regions, must be especially effective in dissipating clouds and checking or modifying their formation. The meteorological results of this may be important, and are worthy of careful study.

In thus venturing to question some of Prof. Langley's inferences I am far from underrating the interest and importance of his researches. On the contrary, I regard the quantitative results he has obtained as especially valuable and opportune, in affording means of testing the above-named and other speculations in solar physics. Similar observations repeated at different elevations would decide, so far as the lower regions of our atmosphere are concerned, whether or not there is any difference in the quantity of heat imparted by the bright and obscure portions of the Sun to our atmosphere. If the differences already observed by Prof. Langley vary in ascending, a new means will be afforded of studying the constitution of the interior of the Sun and its relations to the photosphere. Direct evidence of selective absorption by our atmosphere may thus be obtained, which would go far towards solving one of the crucial solar problems, viz. whether the darker regions are hotter or cooler than the photosphere.

The obscure radiations from the Moon must be absorbed by our atmosphere like those from the sun-spot, and may be sufficently effective to account for the alleged dissipation of clouds by the full moon.

In both cases the climatic influence is greatly heightened by the fact, that all the heat thus absorbed is directly effective in raising the temperature of the air. The action of the absorbed heat in reference to cloud-formation is directly opposite to that of the transmitted solar heat, which by evaporation of surface water supplies additional cloud material, instead of effecting its dissolution.

* *The Fuel of the Sun*, chapters 4 to 10.

Spectroscopic Results for the Rotation of Jupiter and of the Sun, obtained at the Royal Observatory, Greenwich.

(*Communicated by the Astronomer Royal*).

(*Continued from p.* 36 *of the* '*Monthly Notices*' *for November* 1876. *For the explanation of these observations see pp.* 23 *and* 24 *of the above No.*)

Rotation of Jupiter.

Relative motion of East and West limbs in the line of sight deduced from the relative displacement of the F-line.
The slit was tangential to the limb.

Date.	Observer.	No. of Measures.	Weight.	No. of Prisms.	Position-Circle.	Width of Slit.	Line compared	Earth's Motion in Miles per Sec.	Concluded Motion. Measured.	Concluded Motion. Estimated.	Remarks.
					°	d					
1876 June 26	M	1	½	2½	16½	0·5	F	...	+40·5	...	Line very faint; the slit about 0″·6 from the planet's limb.
26	M	1	½	2½	16½	0·7	F	...	+11·9	...	The slit about 0″·4 from the planet's limb.
26	M	1	2	2½	16½	1·7	F	...	+30·4	...	Line much better seen, but still very faint.
								Mean	+29·0		Calculated relative motion +32 miles per second.

Rotation of the Sun.

Dispersion—Ten Compound Prisms; Magnifying Power 36.

1 rev. of Micrometer = 3·3 tenth-mètres, corresponding to 103 miles per second at D
= 2·0 „ „ 72 „ „ *b*

The readings increase towards the blue.

1876, May 30d 4h Observer—Mr. Christie.

Measures of the line 5161·5 (Ångström) near b_4

Micrometer Readings.

Position-Circle 270°. E. Limb. r	W. Limb. r	Displacement. E. Limb—W. Limb. r
(1) ·379	(2) ·335	+0·044
(3) ·374	(4) ·346	+0·028
(5) ·372	(6) ·362	+0·010
(8) ·400	(7) ·366	+0·034
(10) ·402	(9) ·363	+0·039
(12) ·372	(11) ·342	+0·030
(14) ·378	(13) ·358	+0·040

Mean displacement, E. limb—W. limb +0·032
corresponding to +2·30 miles per second

Position-Circle 0°. S. Limb. r	N. Limb. r	S. Limb—N. Limb. r
(1) ·634	(2) ·649	−0·015
(3) ·645	(4) ·635	+0·010
(5) ·610	(6) ·613	−0·003
(7) ·628	(8) ·621	+0·007
(9) ·646	(10) ·639	+0·007

Mean displacement, S. limb—N. limb +0·001
corresponding to +0·07 mile per second

Probable error of 1 measure of displacement = 0r·010 or 0·7 mile per second

Concluded Motion in miles per second.

	Position-Circle.	Line	No. of Obs.	E. Limb—W. Limb. Observed.	Calculated.
1876 May 29	90°	D_1	20	+2·26 ± 0·16	
30	270	D_1	7	+2·26 ± 0·27	+2·35
30	270	near b_4	7	+2·30 ± 0·27	
				S. Limb—N. Limb.	
30	0	D_1	6	+0·02 ± 0·29	
30	0	near b_4	5	+0·07 ± 0·31	

The part of the spectrum observed was in each case at a distance of about 8″ from the limb.

The position-angle of the Sun's N. Pole was 344½°.

The period of rotation has been taken as 25⅓ days.

On the Error of the Tabular Place of Venus on December 8, 1874. By Colonel J. F. Tennant, R.E., F.R.S.

(*From a Letter to E. Dunkin, Esq., F.R.S.*)

After coming here last January I compared the tabular places which I got for the transit of *Venus* by the Astronomer Royal's tables with those I had used. I found that the printed tables were 2″·03 in excess of mine in the R.A. of the Sun. I was anxious to find the source of this before saying anything about it, but many interruptions have occurred. All I can say is, that making a third computation in the same way that Captain Campbell and I had deduced all the formulæ, I got results only numerically different in the last place from those I had got before. It is, nevertheless, a fact that the Tables published at Greenwich are right; but it is curious that Captain Campbell and I, using an arithmometer in computations, should have entirely agreed, and that a third calculation with a different instrument gave the same result, all *three being wrong*.

Now to the effects. In the *Monthly Notices* of November 1875, you gave errors of the place of *Venus*. These are affected in R.A., and I have already sent you a correction to the difference in N.P.D. With the new correction they should stand :—

Correction to (R.A. ♀) − (R.A. ⊙) = + 6″·50 − 0″·071 dL − 0·989 $d\pi$,

„ (N.P.D. ♀) − (N.P.D. ⊙) = − 2″·24 − 0″·017 dL − 2·689 $d\pi$.

Calcutta,
1876, *November* 3.

On some Phenomena of the Internal Contacts common to the Transits of Venus, observed in 1769 *and* 1874, *and some Remarks thereon.* By E. J. Stone, M.A., F.R.S., Her Majesty's Astronomer, Cape of Good Hope.

When the Observations of the Transit of *Venus*, 1874, are published in detail, I hope to examine the observations of the internal contacts with reference to their bearing on our determinations of the Sun's distance. I should have preferred not to have written upon the subject before the whole of the materials collected were available. But an impression has been slowly hardened into a conviction that none of the skilled observers, who, in 1874, observed the internal contacts with good instruments, saw the so-called black-drop phenomenon. Such an impression is, I am quite sure, unfounded, and it is perhaps desirable that this should be pointed out. The essential facts

are in this case, as in too many other cases of the kind, becoming veiled in a mist of words.

Undoubtedly, if we are prepared to assume, on *à priori* grounds, that what a good observer, provided with a good instrument, should see, is the simple geometrical contact at a point, and that any observer who has seen more, must be either an unskilled observer, or have been provided with a defective instrument, we shall find no difficulty in establishing, to our own satisfaction, the conclusion which I have mentioned. But, on the other hand, there will be no difficulty in showing that observers have seen the "black-drop," who would have been considered observers of skill, and to have been provided with good instruments, had their observations not failed to satisfy some preconceived ideas of the nature of the phenomena which should be presented at the internal contacts. And here, perhaps, I may be permitted to add a word or two in the way of a caution. Nothing whatever depends upon the phrase "black-drop" or "black-drop phenomenon." This is merely the way in which one observer, in 1769, thought proper to describe the lingering nature of the contact, which is the cause of the only systematic error to be feared in a discussion of durations derived from internal contacts, and the cause of the principal systematic error in a general discussion of contact observations.

Whether the observer prefers to speak of the disturbance of the apparent limb of the Sun near the point of contact, as an "ombre," a "black-drop," a "ligament," a "thread," or merely to assert generally, that "the contact was gradually established," is a point of very little importance.

The important point connected with these protracted contacts is the sensible time over which the contact extends; this renders no longer admissible the assumption that all contacts take place with the same angular separation of the centres of *Venus* and the Sun. For if, at ingress for example, after the geometrical contact appeared to be established there still continued for thirty or more seconds some disturbance of the Sun's apparent limb, near the point of contact; the time at which the observer saw the geometrical contact, and the time at which he last saw any disturbance of the Sun's limb near the point of contact, cannot be combined together in any mathematical investigation without allowing for the relative change in the positions of *Venus* and the Sun in the thirty or more seconds which elapsed between the observations, or without accepting residual errors of fifteen or more seconds. And when we have to combine observations of contacts made by different observers, we cannot proceed a single step without an assumption. We have to assume that contacts of apparently the same class take place at all the different stations with the same angular separations of the centres. Unless this assumption is approximately true for some class of contacts, attempts to determine a value of the solar parallax from contact observations in a transit of *Venus* must entirely fail. It is, how-

ever, a pure assumption, which may or may not turn out to be correct. Like other assumptions of the class, it can only be proved or disproved by logically tracing its consequences, and comparing them with the known facts, which are the observations made at the different stations.

But suppose, as will be found to be the case, that some of the observers, who have seen the contact of a lingering or protracted character, have preferred to give as the time of contact the times when they saw the so-called geometrical contact, whilst others have preferred to give the times at which at ingress they last saw, or at egress first saw, any disturbance of the Sun's apparent limb near the point of contact. How are these incongruous observations to be combined? It is clearly useless to attempt to combine them under a single assumption, that all the contacts took place at the same angular separation of the centres of the planets, unless we are prepared to admit, in the comparison of our theory with the observations, residual errors, or so-called errors of observation, at least equal to one-half the time over which the contacts have been observed to be prolonged. Enough is already known of the observations made in 1874 to justify the statement that these contact observations thus treated cannot lead to any accurate value of the solar parallax without the adoption of some method of selection of materials equivalent to the rejection of a considerable number of observations. If we were sufficiently acquainted with the causes which produce the appearances of prolonged contact, and were in possession of the requisite data upon which to base the necessary calculations, our proper course, to utilise the whole of the contacts, would be to adopt as our standard phase the *real contact*, an expression which in the case supposed would have a clear, distinct, and definite meaning, and to apply corrections to the times of the observed contacts to reduce them to the corresponding times of real contact. But an error of a tenth of a second of arc in assumed difference of semi-diameter is equivalent to an error of three seconds in time in the observed contact. I am afraid that we possess neither the theoretical knowledge nor the requisite data to render such a method available within allowable limits of error. If such be the case, we are thrown back upon the method which I adopted in my paper on the "Rediscussion of the Transit of *Venus*, 1769." Just as we assume that all the observations of geometrical or apparent contact are seen at some definite angular separation of the centres of the planets, so must we assume that all the observations of the first and last disturbance of the Sun's limb near the point of contact take place at a definite angular separation of the centres. The second assumption is as legitimate as the first; its approximate truth can only be proved or disproved in the same way, by logically tracing the consequences of the assumption and comparing them with the observations. In practice we must expect to find both of these assumptions satisfied with greater or less accuracy, according as

greater or less uniformity has been secured in the instrumental equipment of the observers, the brightness of the field of view in which the observations were made, and in points to which the attention of observers was specially directed.

In the transit of 1769 there were ten complete observations of duration from internal contacts. The smallness of the residual errors in my rediscussion of these observations proves that, so far as they are concerned, the assumptions to which I have called attention were at least very approximately satisfied. Whether such will be the case with the contact observations of 1874 remains to be seen. The similarity of the phenomena of the contacts observed in 1769 and 1874, to which I shall call attention, may probably have some effect in reassuring astronomers of the objective character of the phenomena, and may thus render more general the acceptance, as at least *à priori* probable, which is all that is required, of the assumptions which I have already mentioned and of the other supplementary assumptions of the same class, which the want of uniformity in instrumental equipment and attempts to introduce subsidiary phases, which appears to have been made in 1874, may be found to have rendered necessary.

I have entered into these details because they must be understood before the importance of the remarks made by the observers can be appreciated, and because the principle upon which my "Rediscussion of the Transit of *Venus*, 1769," is based has been entirely misunderstood by one popular writer on the subject.

In the present paper I shall make no further reference to my own observations than to say, that I saw the so-called penumbra of Cook and Green. The light of the penumbra was, however, so faint in comparison with the direct light of the Sun that, when attention was concentrated on the contact all distinction between the central darker portions of the planet were lost sight of. The disturbance of the Sun's limb at egress appeared to me to commence some considerable time before the apparent contact, when, in fact, the limb of *Venus* was more than the second of arc from the apparent limb of the Sun. At first, however, the disturbance was of a very slight character. The appearance at first presented was something like that of a hair in vibration before the eyes. Such an appearance I consider as an indication that some of the pencils of light which had previously reached the eye from the Sun's disk were more or less interrupted by the planet *Venus* or its atmosphere. The appearance of disturbances soon assumed a definite character; and I should be content to describe the connection between the limbs of *Venus* and the Sun as a ligament. The apparent contact was rendered more difficult than would otherwise have been the case by the appearance of this ligament, or, in other words, by the disappearance of a sensible portion of the Sun's limb near the point of contact. The observations of Contact were made in a rather bright field of view.

The definition was good, the sky perfectly free from clouds, and there was but little wind.

I am aware that it has been stated that my observations have been affected by my theoretical views; and, as the same charge might probably be brought against some other observations to which I could appeal, I shall, for my present purpose, confine myself to observations made by the French observers. The instrumental equipment of these observers was supposed, before the transit, to be sufficiently perfect to prevent any instrumental defects affecting their observations. It is clear, also, that the observers, if biassed at all, were not biassed in favour of any views of mine, and their observations have been printed in some detail, and are available for general examination. The observations to which I shall first appeal to prove that the phenomenon of the black-drop did present itself in the transit of 1874 are those made by M. Janssen at Nagasaki. M. Janssen, in describing the contact at ingress in the *Annuaire*, 1876, says that "le premier contact est obtenu par M. Tisseraud et par moi. Dans l'equatorial de 8 pouces, dont la lunette est très-bonne, l'image de *Vénus* se montre très-ronde, bien terminée, et la marche relative du disque de la planète, par rapport au disque solaire, s'exécuta géométriquement sans aucune apparence de ligament ni de goutte." Nothing can apparently be clearer than the statement of M. Janssen; and it has therefore been seized upon as conclusive evidence against the black-drop in 1874. But M. Janssen continues his account of his observations as follows:—"Mais il s'ecoula un temps assez long entre le moment où le disque de *Vénus* paraissait tangent intérieurement au disque du Soleil et celui de l'apparition du filet lumineux." This prolongation of the disturbance of the Sun's limb near the point of contact after the geometrical contact, for a "temps assez long" constitutes what other observers have called, and I should call, the black-drop. M. Janssen has apparently confined himself to giving the time at which he saw the geometrical contact; but he might, had he pleased, have given us instead the time later by the "temps assez long" which corresponded to the first appearance of the "filet lumineux." To show that this is really the point upon which controversy has hinged, I will give, for comparison with Janssen's Observations, those made by Wales and Dymond in 1769.

These observers state, "We took for the instant of last internal contact the time when the least visible thread of light appeared behind the subsequent limb of *Venus*; but before that time *Venus's* limb seemed within that of the Sun." It is clear, therefore, that Wales and Dymond in 1769 and Janssen in 1874 saw similar phenomena; but that Wales and Dymond preferred to give the time corresponding to the last appearance of any disturbance, or to the first appearance of the "filet lumineux"; whilst Janssen preferred to give the time corresponding to the geometrical contact. In the *Annuaire* for 1876—where are also

some very important remarks by M. Janssen which appear to prove, in the most conclusive manner, the objective character of the phenomenon—M. Janssen mentions that, after the geometrical contact, he proceeded to take photographs, and that these photographs showed that the connection between the limbs of *Venus* and the Sun had not ceased.

At the egress, M. Janssen's observations would appear to have been picked up through clouds, and the details are wanting.

The evidence thus afforded by M. Janssen of the existence of the black-drop in 1874 is not only of the most conclusive character, but it is above any suspicion of being the result of preconceived ideas. It would appear that the observer had some misconception of the real character of the black-drop, and his evidence in favour of its existence is given quite unconsciously. The important fact mentioned by M. Janssen, that after his observations of the geometrical contact the photographs still showed that the connection between the limbs had not ceased, proves that the prolonged contact is not a mere optical delusion, "for photographs have no nerves," to whatever other sources of error they may be liable. It may be remembered that soon after the transit great stress was laid upon the supposed fact that Capt. Abney had secured a photograph of the contact, and that there was no appearance of any black-drop. It was, however, found, on examination, that the contact had not even commenced at the time the photograph was taken; but the impression made by the original statement, although admitted to be erroneous, has never, I fear, been removed. The contradictions of such statements do not usually receive the same publicity, or attract the same attention, as the original statements.

The next observations to which I shall refer are those made by Capt. Mouchez at the Island of St. Paul. Capt. Mouchez's telescope had an aperture of eight French inches. The following account of these observations is taken from the *Annuaire*, 1876:

Speaking of the internal contact at ingress, Capt. Mouchez says:—"Les nuages devenaient de plus en plus rares, le ciel plus transparent, les images d'une très-grande netteté. Un quart d'heure environ après le premier contact, quand la moitié de la planète était encore hors du Soleil, j'aperçus subitement tout le disque entier de *Vénus* dessiné par une pâle auréole, plus brillante dans le voisinage du Soleil qu'au sommet de la planète. Pour bien constater que je n'étais pas le jouet d'une illusion, je pris immédiatement des mesures micrométriques du diamètre de *Vénus*, dans le sens de la ligne des centres et dans le sens perpendiculaire; bien que le premier de ces diamètres fût encore à moitié hors du Soleil, et limité extérieurement seulement par l'auréole, j'obtins, à 1 seconde près, exactement la même valeur: c'était donc bien le disque entier très-net de la planète que j'aperçevais.

"Cette apparition, aussi rémarquable qu'inattendue, peut sans doute être attribuée en partie à l'atmosphère solaire rendue

visible par contraste, en partie aussi par l'atmosphère de *Vénus*. Le ciel était devenu si pur à la suite de la tempête et l'auréole était si brillante, qu'on peut voir sur nos photographies des traces de ce curieux phénomène. Le deuxième contact fut observé dans de bonnes conditions vers $7^h\ 30^m$; mais, au lieu de voir l'apparence de la goutte noire, qui a pour effet de tenir les deux cornes encore séparées après le contact, j'ai vu le phénomène tout contraire: l'arc lumineux de l'auréole réunissait ces deux cornes avant le contact. Nous nous trouvions évidemment dans des conditions exceptionnellement favorables sous le rapport de la pureté de l'atmosphère et de l'excellence de notre lunette."

It is to be regretted that Capt. Mouchez has not stated more clearly whether there was or was not any disturbance of the Sun's limb after the contact.

It should be noticed that the circumstances under which the auréole first presented itself to Capt. Mouchez are exactly similar to those under which it first appeared to Wales and Dymond in 1769.

These observers state, "Soon after *Venus* was half immerged, a bright crescent or rim of light encompassed all that part of the circumference which was off the Sun, thereby rendering her whole periphery visible. This continued very bright until within a few minutes of the internal contact, and then vanished away gradually." Upon these points see the observations of M. Heraud, which will be subsequently given. The observations of Capt. Mouchez are chiefly valuable, for my present purpose, for the light which they throw upon Cook and Green's observations in 1769. The auréole is nothing more than Cook and Green's penumbra. I shall have something more to say upon this point with respect to MM. Heraud and Bonifay's observations.

The following accounts of the observations of MM. Heraud and Bonifay at Saïgon are extracted from the *Comptes Rendus* (25 Janvier 1875):—

"*Observation de M. Heraud.*

"Objectif de 160 millimètres: grossissement, 155.

"*Entrée.*—A $21^h\ 17^m$ la planète étant déjà entrée de plus de deux tiers, je remarque que la partie extérieure de son limbe est nettement indiquée par un filet lumineux pâle, qui, réuni aux franges de l'image intérieure, dessine un rond parfait. Ne m'attendant pas à ce phénomène, je ne puis noter l'instant précis de son apparition; l'heure ci-dessus est donnée à 1 minute près.

"L'échancrure s'arrondit de plus en plus, je suis le rapprochement régulier des pointes brillantes du croissant solaire; à un moment donné, ces pointes me paraissent immobiles. Je ne vois plus la petite auréole extérieure, la partie noire de l'échancrure me paraît absolument ronde et tangente à la ligne fictive qui fermerait le bord du Soleil. Je donne un top et, perdant de

vue les franges, je crois un moment que le contact s'est produit et qu'il est perturbé par la goutte noire. Je regarde avec attention, et 20 secondes plus tard je note l'apparition entre l'image noire et le fond du ciel d'une lueur très-pâle teintée de noir en son milieu ; cette lueur, qui arrive comme un transition entre l'obscurité et la lumière, s'agrandit et s'anime, et, en même temps, la petite tache noire devient plus petite. Je la signale comme formant une sorte de pont obscur entre le bord des astres qu'elle laisse cependant distincts ; elle disparaît presque aussitôt, et le filet lumineux est dépouillé de tout trouble, les franges reprennent leur netteté autour de la planète. Les phénomènes de l'entrée sont accomplis. . . . .

" *Sortie.*—Les images sont moins calmes que dans la matinée, mais encore nettes ; l'image de la planète est toujours bien frangée.

" La planète se rapprochant de plus en plus, je suis attentivement le filet lumineux. Je signale successivement l'apparition d'un filet lumineux très-faible, puis celle d'un filet lumineux presque nul. Le filet très-pâle est teinté de noir comme dans la matinée, et ressemble à ce qu'il était lors de son apparition à l'entrée ; mais je n'ai pas revu le ligament plus net signalé dans la première observation. Toute apparence lumineuse disparaît ; c'est *l'instant du contact* ; la partie noire de l'image de *Vénus* est à une distance appréciable du bord du Soleil ; peu d'instants après, cette distance me paraît nulle, et l'échancrure noire semble tangente au bord du Soleil ; les cornes du croissant s'éloignent, mais sans prendre tout de suite l'acuïté qui correspond à une intersection géométrique ; je les signale comme étant un peu émoussées, et ce n'est que trente-trois secondes plus tard que l'échancrure me paraît bien nette.

" *Observation de M. Bonifay.*

" La lunette dont je me suis servi est de la maison Secrétan ; elle a un objectif de 55 millimètres d'ouverture et un grossissement de 63.

" La mise au point sur les étoiles et sur les taches du Soleil s'opérait sans difficulté et donnait des images très-nettes. C'est cette mise au point qui été adoptée pour l'observation.

" J'ai pu après l'observation, déterminer, avec la lunette méridienne prise comme collimateur, la mise au point sur le plan focal. Elle exigeait que le tube de l'oculaire fût légèrement moins enfoncé que la mise au point sur le cercle maximum d'aberration.

" L'observation a été faite dans le Jardin de l'Observatoire, avec cette lunette, montée en altazimut, sur une table massive, à 20 mètres à l'est de l'Observatoire.

" *Entrée.*—Quand la planète se projette sur le Soleil, je constate que l'image est noire, de teinte parfaitement uniforme et à contours très-nets ; elle conserve ce même aspect pendant toute l'observation.

"A 21^h 18^m, temps moyen de Saïgon, le contour de *Vénus* extérieur au disque solaire s'illumine légèrement, à commencer par le bas de l'image, qui reste constamment plus visible que le haut. La circonférence planétaire paraît ainsi complétée d'une manière très-visible sur le ciel par cet arc lumineux qui semble la continuer exactement.

"Cet effet subsiste quand la planète avance; peu à peu le disque solaire, entre les bords voisins des deux astres, devient de plus en plus obscur à côté du futur point de contact. Quand le moment du contact approche, on ne distingue plus le bord du Soleil qui jusqu'alors se prolongeait nettement jusqu'au disque planétaire; les deux cornes de l'echancrure sont séparées de *Vénus* par un intervalle obscur; mais je continue à voir le bord de la planète, qui reste légèrement lumineuse. Cette circonférence lumineuse me paraît tangente au bord du Soleil, prolongé par la pensée dans l'ombre; c'est le moment qui me semble être celui du contact. La planète s'éloigne du bord du Soleil, laissant obscur l'intervalle qui les sépare; un filet lumineux vient compléter la circonférence du Soleil en réunissant les deux cornes de l'echancrure: l'ombre qui persiste entre *Vénus* et le Soleil n'est complètement dissipée que vingt-trois secondes plus tard. . . . .

"*Sortie.*—Les images sont ondulantes; néanmoins comme le matin, les contours de la planète sont bien tranchés; sa teinte est uniformément noire. Quand *Vénus* s'approche du Soleil, une ombre s'étend entre les deux astres; le bord du Soleil se rompt en deux cornes au point où doit s'effectuer le sortie, et les contours de deux astres en ce point deviennent invisibles. Je ne puis juger du moment du contact qu'en l'appréciant de mon mieux, en continuant par la pensée les parties invisibles des circonférences; de même dans une observation au sextant d'une hauteur de Soleil, si le bout de l'astre est en partie masqué par un petit nuage, il arrive qu'on cherche à obtenir le contact au juger.

"Quand la planète commence à émerger j'examine si sa circonférence devient lumineuse, comme le matin: le phénomène ne paraît pas se reproduire. L'échancrure diminue de plus en plus; peu à peu les ondulations du Soleil rendent son observation difficile, elle devient enfin invisible. . . . ."

In these observations we have all the phenomena observed in 1769 by Wales, Dymond, Hell, Chappe, Cook, and Green, reproduced with modern instruments, and in the eyes of observers who were certainly not biassed in favour of any views of mine. The fringe is here again the penumbra of Cook and Green. It may, perhaps, be remembered that Cook and Green, in 1769, observed the geometrical contacts of a penumbra as dark, or nearly as dark, as that of the planet, and also the geometrical "contacts of the dark body of *Venus*." The following remarks of M. Heraud are important as bearing on these points, and on the gradual establishment of the contact:

"L'apparition du filet lumineux a été pour ainsi dire graduelle; de plus, comme je l'ai dit dans le procès-verbal, la sépa-

ration de l'image noire et des franges était plus nette, aussi nette au moins que la séparation des franges et l'image lumineuse du Soleil, et j'ai eu quelque peine à me défendre de considérer comme le bord de *Vénus* la limite de l'image noire, d'autant plus que ces franges n'ont reparu que très-lentement au point où le contact s'était fait : et c'est ainsi que j'avais noté d'abord à l'entrée le moment où la partie noire de l'echancrure me paraissait ronde et tangent au Soleil."

It will be seen, therefore, that Mons. Heraud had some difficulty in refraining from making an observation of the contact with the so-called "dark body of *Vénus*," like that made by Cook and Green in 1769, in addition to the geometrical contact of the outer boundary of *Vénus*. M. Heraud also remarks, "Pour M. Bonifay, l'arc lumineux a persisté jusqu'au moment du contact et peut-être jusqu'a près ce moment, tandis que pour moi il avait disparu : je dois dire que, preoccupé de l'observation du contact, j'avais un peu perdu de vue la petite auréole, et je n'ai pas noté l'instant de sa disparition." M. Heraud's remarks on this point agree with those made by Wales and Dymond ; and I should be prepared to accept them as representing fairly my own observations.

*In discussions of Cook's and Green's observations before the appearance of my paper, the geometrical contacts of the penumbra as dark, or nearly as dark, as the planet, were taken as "real contacts" "the first or last appearances of any disturbance at the Sun's limb near the point of contact," or "the first or last appearance of the least visible thread of light." The contacts with the "dark body of *Venus*" were taken as the apparent contacts. These interpretations appeared to me as unsatisfactory. If the penumbra was as dark, or nearly as dark, as the planet, the observers who observed geometrical contacts without discovering any distinction between the penumbra, or fringe, and the central darker portions of the planet, must have observed also contacts with the penumbra. In this case Cook's geometrical contacts of the penumbra, instead of being real contacts, were apparent or geometrical contacts.

A reference to the drawings, of which copies are given in my papers, will show that at the time of the geometrical contacts with the "dark body of *Venus*," the disturbance of the Sun's limb extended over nearly a diameter of *Venus*, and that the true geometrical contact could not be thus intended. But the point which, as much as any other, influenced me, in the interpretation put upon these observations, was the remark made by Cook, that "the contacts were made gradually, and the time noted was when the thread of light was wholly broke by the penumbra." This showed that Cook and Green had seen the disturbances of the Sun's limb, before at egress, and, after at ingress, the penumbral contacts. It was clear, therefore, that Cook and Green's

* This sentence is printed according to the MS., but some correction is perhaps required.—[Ed.]

contacts could not be real contacts in the same sense as those made by Wales and Dymond were. The confirmation of my views, on the interpretation to be put on Cook's and Green's observations, afforded by the observations of the French observers in 1874, is remarkable.

Since the appearance of my paper I am not aware that any astronomer has seriously maintained the correctness of the interpretations which had previously been held with respect to the nature of the contacts observed by Cook and Green. One astronomer doubted the goodness of the observations, and was inclined to attribute the appearance of the penumbra to instrumental defects. The observations of the French observers appear to show that there is no longer any ground for such a suspicion, and that the observations of 1769 are as good, or very nearly indeed as good, as the corresponding observations of 1874. Any one who will take the trouble to study my papers in the *Monthly Notices* for 1868, October, will see that the interpretation which I put upon the observations of Cook and Green is the most important point in my work. In fact, the only other step of any importance was the rectification of a similar mistake in Hell's observations, indicated by the fact that different interpretations had been assigned to the word "contactus," at the ingress and egress.

Small and unimportant changes are easily enough made in all such work. But when any astronomer will prove that one or more of the twenty internal contacts included in the ten complete durations observed in 1769 has been erroneously interpreted by me, and will show that when my erroneous interpretation is removed and the correct interpretation carried into the equations of condition, the ten durations are fairly represented, and that the value of the solar parallax deduced from the equations is no longer my value $8''{\cdot}91$, or one differing from it by $\pm\, 0''{\cdot}07$, but is Encke's value $8''{\cdot}58$, or one differing from it by $\pm\, 0''{\cdot}07$—then I shall be quite prepared to admit that my work has been without value. Before this is done I can only meet general statements that the work is not accepted, by asking those who assert that such is the case to go one step further and prove that it ought not to be accepted. If the work is inaccurate, the proof should be easy; but no proof of the kind has yet been given.

Royal Observatory, Cape of Good Hope,
1876, *November* 6.

On some Diffraction Experiments of M. Ch. André with reference to Astronomical Instruments, and the General Theory of this Diffraction. By the Rev. S. J. Perry.

My attention was first called to these experiments during a visit I paid this year to France; and whilst in Paris I had the pleasure of observing with the instruments, and of noting the extreme care with which the experiments are being made under the most favourable circumstances. Some of the experiments are confirmatory of results already noted by other astronomers, as by Sir W. and Sir J. Herschel and the Rev. W. Dawes, whilst others, as well as the complete theory of the subject, are, as far as I am aware, due entirely to the labours of M. André.

The image of a luminous point in the focus of a telescope is known to be a central disk surrounded by bright rings, the diameter of the disk varying inversely as the aperture of the object-glass or mirror employed. Hence the limitation to the separating power of a given instrument. If, however, the centre of the field is covered, the diameter of the focal disk diminishes as the diaphragm is increased. The separating power is thus augmented, but the ratio of the intensity of the bright rings to that of the central disk is proportionately increased.

If we draw a curve, whose ordinates represent the intensity of the light of each point of the image, and whose abscissas are the angular distances of these points from the axis of the telescope, this intensity curve, drawn for any particular aperture, will hold good for any other aperture, provided the unit for the abscissas is made inversely proportional to the aperture. A revolution of this intensity curve round its vertical axis will form what M. André calls the Solid of Diffraction; and if the axis of this solid coincides with that of the telescope, the quantity of light on any element of the focal plane is proportional to the cylindrical portion of the volume of the solid of diffraction, the base of the right cylinder being the given element.*

When the object is of finite dimensions, instead of being a luminous point, there is no interference of the rays from the different points, but the intensity of the light at any part of the image is the sum of the intensities of the separate elements. Hence we obtain the following theorem:—If the solid of diffraction, corresponding to the aperture employed, be placed so that its axis, which is perpendicular to the focal plane, passes through a point (m), the cylindrical portion of the solid, comprised within the geometrical image of the object, measures the intensity of (m).

Applying this theorem to objects of different dimensions, we find that the intensity of the image of a brilliant straight line is measured by the surface of a section of the diffraction solid;

* The width of the solid of diffraction varies inversely as the aperture of the telescope.

and that if the straight line is widened into a band, the measure of the intensity of the image is the volume of the same solid comprised between two sections, which widen out in the same proportion as the luminous band. Taking the more practical case of an object whose diameter is so considerable in every direction that we may consider the boundary lines as straight, we easily see that the focal image consists of two parts:

One similar to its geometrical image, depending on its form and apparent dimensions, but increasing with the aperture; the intensity being constant and maximum throughout:

The other overlapping and surrounding the first, its shape varying with that of the object, but its angular dimensions depending solely on the aperture. The intensity of this portion of the focal image, decreasing progressively, is reduced to one half at the limits of the geometrical image, and then quickly becomes insensible.

To find the intensity at the different points of this zone of diffracted light, which surrounds the geometrical image of any object of sufficient apparent magnitude, we must calculate the successive portions of the volume of the solid of diffraction intersected by a plane, which keeps parallel to the axis of the solid, whilst it moves from one edge of the zone to the other. As the extent of the zone of diffracted light bears a constant ratio to the maximum intensity of the focal image, the diameter of any body of sufficient brilliancy, observed on the same background, varies with the aperture. Thus for a 4-in. aperture the angular measure of the zone of diffracted light is $1''{\cdot}4$, on the supposition that light, reduced to $\frac{1}{30}$th of the central portion, ceases to affect the retina sensibly. Diameters are thus increased $2''{\cdot}8$ by a 4-in. telescope. This correction, called the Constant of Instrumental Diffraction, differs from the separating power of the telescope, and varies with the aperture of the telescope and the brilliancy of the object.

In order to verify experimentally these theoretical results, a hole was made in a piece of silvered copper, and a strong light from a Drummond lamp thrown upon it. Observing this by direct and reflected light, differences of $5''{\cdot}82$, $8''{\cdot}87$, and $13''{\cdot}60$, were found with apertures of 6·5, 2·9, and 1·7 centimeters.

A more accurate verification was made with a piece of blackened brass, in which two rectangular apertures had been carefully cut with bevelled edges, 3 centimeters in height by 1·5 in breath, and 1·5 apart. Direct light was alone used, and with apertures of 6·5, 3·0, and 1·7 centimeters, the exterior diameters were found to be $121''{\cdot}15$, $122''{\cdot}17$, and $125''{\cdot}86$, and the interior ones only $36''{\cdot}13$, $34''{\cdot}48$, and $31''{\cdot}46$, respectively.

Measures of the diameters of *Mercury* and *Venus*, whilst the planets were crossing the solar disk, also lead to the same conclusions.

Thus, in the Transit of—

		Observer.	Instrument.	Aperture.	Diameter measured.	Reduced to Unity of Dist.
Mercury	4, 5 May 1832	Bessel	Heliometer	0·160	11·987	6·697
		Gambart	Equatoreal	0·070	9·290	5·184
„	8 May 1845	Mitchel	„	0·265	11·580	6·522
„	„	Mädler	„	0·224	11·754	6·542
„	„	Schmidt	„	0·068	10·853	6·057
„	8, 9 Nov. 1848	Arago	„	0·162	10·155	
„	„	Dawes	„	0·102	9·389	
„	„	„	„	0·072	9·369	
„	4 Nov. 1868	Wolf	„	0·204	9·430	6·367
„	„	Plummer	„	0·165	9·001	6·077
„	„	Struve	„	0·064	6·840	4·618
Venus	8 Dec. 1874	Mouchez	„	0·217	64·992	16·992
„	„	Tennant	„	0·152	63·948	16·904

Calculating the constant of instrumental diffraction for a 4-in. telescope from the last observations of *Mercury* and *Venus*, we obtain a mean value of $2''\cdot634$, which agrees closely with the theoretical value of $2''\cdot80$. If we compare the above diameters with measures taken at other times, we find that the diameter invariably decreases with the brilliancy of the background.

The corrections applied to the lunar diameter, when reduced from occultations of stars by the Moon, observed at the Royal Observatory with the East Equatoreal, affords another confirmation of the correctness of the preceding conclusions. The correction adopted for stars observed at the dark limb is $-2''\cdot54$, and at the bright limb $-0''\cdot80$; the difference is, therefore, $1''\cdot74$, whilst theory would give for the same instrument $1''\cdot55$.

When we increase the separating power of a lens by means of a central diaphragm, the constant of instrumental diffraction is increased, on account of a change in the form of the solid of diffraction, consisting in an increase of the relative volume of the parts external to the first minimum of intensity. If the aperture is thus reduced to $\frac{1}{2}$, the decrease of light ceases to be continuous, and the constant of instrumental diffraction increases for a 4-in. telescope from $2''\cdot8$ to $4''\cdot0$. This has been verified by observations of the Moon and also of artificial objects.

As the mathematical theory of the subject has been worked out fully in a published essay, it would be out of place to enter into the question here, especially as this short notice of the investigations of M. André is given mainly as an introduction to a second paper on a kindred subject.

On the Phenomena exhibited by a Planet in its Transit across the Solar Disk, from Observations made by M. Ch. André.

By the Rev. S. J. Perry.

The results contained in this paper have a special interest at the present time, as the experiments were made with the direct view of securing accuracy of observation at the transit of *Venus* in 1882. We have but a short time for making all the necessary preliminary preparations for this, our only remaining chance of testing the value of the transit of *Venus* methods for determining the solar parallax, and it is, therefore, well to learn what is being done elsewhere both as a guidance and as a spur.

The expedition of the Italians to India in 1874 was undertaken mainly with the object of testing the spectroscopic methods of observing the coming transit, and the volume printed by Signor Tacchini shows the success of their endeavours, and serves as an admirable guide for those who would prepare themselves for a fruitful campaign in 1882. Last summer I found that my own spectroscopes gave very satisfactory results with one of the methods; and I purpose examining the others more in detail and under varying circumstances during the coming year.

The experiments of M. André are being made under the most advantageous circumstances. The splendid subterranean basement of the École Normale at Paris is placed at his disposal, and there, in a passage considerably more than 100 meters in length, he has established his apparatus. This consists of a model of the transit of an inferior planet; the planet being moved by clockwork, and passing across the bright field backwards and forwards, without the necessity of any personal adjustment, and registering each contact electrically on a chronograph. The bright field which represents the Sun is illuminated by a Drummond light, and as a supply of oxygen is always at hand, a special pipe having been kindly laid down by M. H. Ste. Claire de Ville from his chemical laboratory overhead, no delay or loss of time is ever experienced; an advantage that will be fully appreciated by those who have made similar observations in the open air, with the Sun itself as the source of light. Three telescopes are mounted at the distance of about 100 meters from the model, and the chronograph is established in an adjoining passage. The method of observing is too obvious and too familiar to need any explanation here. It will suffice to give at once the conclusions at which M. André has arrived, which I will take from a letter received from him towards the end of last month.

1. The bridge, black ligament, or black drop, as it is variously called, is a necessary phenomenon under certain circumstances, and not merely accidental. With an unchanging source of light of sufficient brilliancy the angular dimensions of

the ligament are inversely proportional to the diameter of the object-glass, and with an aperture of 5-inches, the drop is scarcely perceptible. Diaphragms should never be used when observing *Venus* in transit, as the apparent dimensions of the drop are augmented by any increase in the focal length.

2. It is always possible to get rid of the ligament, and reduce the phenomena to geometrical contacts, either

(*a*) by reducing sufficiently the intensity of the source of light, or augmenting the absorbing power of the dark-glass employed; or again

(*b*) by covering the object-glass with a network diaphragm, composed of rings alternately full and empty, and all very thin, but bearing a certain proportion to the focal length of the lens.

3. The defect of imperfect focussing has but little influence on the result, the phenomena changing little as long as a fair focus is obtained.

4. Atmospheric undulations, on the other hand, have a considerable influence. They tend to give the image of the planet every description of strange form, and to them are due the contacts at several points at the same time, of which so much has been said. These are entirely got rid of by coating one of the surfaces of the object-glass with a thin film of silver.

5. Even when the object-glass is so small, and the dark-glass so thin, that the ligament is still present, the observation may still by good training be made very precisely. One phase of the phenomenon is simultaneous for all apertures, and can be estimated without difficulty to within $0^s{\cdot}75$ for interior contact at ingress, and $1^s{\cdot}5$ at egress, thus leaving a probable total error of $2^s{\cdot}25$ in the observed duration of the transit.

6. In order to secure the solar parallax to within $0''{\cdot}01$, it suffices that the error in the duration of transit should not exceed 5^s; a small telescope may, therefore, serve to determine this parallax to within $0''{\cdot}005$. Halley was not then mistaken in his estimate of the great value of his method.

7. The above results agree perfectly with the theory of instrumental diffraction, and can be rigorously deduced from it. To effect this, it is only necessary to draw the geometrical phenomenon as it presents itself at the different epochs of the transit, and to measure the portion of the volume of the solid of diffraction comprised between *Venus* and the Sun's edge. M. André has made the calculations for the epochs 10^s, 5^s, 0^s, and from the beginning and end of the transit.

8. The phenomena are the same for *Mercury* as for *Venus*, but the magnifying power must be greater for *Mercury*, almost double.

The difficulties that have so far surrounded the observation of the transit of *Venus* have arisen mainly from the preconceived idea expressed in the sole word "Contact." The phenomenon could have been geometrical contact only in a restricted

number of cases; and certainly our best results are such remarks as those of F. Hell, when he describes the breaking of the bright line; his words are clear, frank, and precise.

Besides giving the telescope a sufficient aperture, the maximum amount of light should also be obtained. It would be well, therefore, if possible to determine accurately beforehand the exact thickness of dark-glasses most suitable for the observation of the coming transit of *Venus*, and this might be obtained experimentally at the previous transit of *Mercury*. This last question of the dark-glass, which seems hitherto to have been considered but of little moment, is in reality one of the most important conditions in the observation.

If I might add one remark to these useful conclusions of M. André, I should think it would be well to make experiments also as to the tint of coloured glass best suited for diminishing the light of the photosphere, without altering considerably that of the chromosphere, in order to give some value to the first external contact. Dr. Janssen has already shown us the feasibility of this plan, and demonstrated that the spectroscope is not required in order to see *Venus* on the bright background of the chromosphere.

On the Orbit of a Centauri. By A. Marth, Esq.

(*From a Letter to E. Dunkin, Esq., F.R.S.*)

At the close of the last meeting of the Royal Astronomical Society you proposed that I should furnish in writing the substance of some remarks on the Double Star *a Centauri*, which I had been permitted to address to the meeting just before.

Though it has been pointed out some years ago that the comes of *a Centauri* would reach its apparent periastre in the course of the year 1875; and though the great importance of securing series of good observations during the preceding and following years is recognised and appreciated by all who keep themselves informed on the subject of binary stars; there seems cause to apprehend that the observers in the Southern Hemisphere, who alone are favourably situated for making such observations, have persisted in their strange neglect of this the most interesting of all double stars. Thinking that it might do some good, and could do no harm, if the subject were brought before the meeting, and if the personal friends of the Southern observers were appealed to for their friendly assistance, I asked leave to occupy a few minutes before the adjournment with a short statement of the circumstances of the case, and to exhibit a diagram which would render any detailed explanation superfluous. In order to enable readers of this letter to reconstruct the diagram (and they will find it far more instructive and satisfactory if they themselves

draw it on a larger scale on squared paper than if they got it ready printed on a small scale), I subjoin the rectangular co-ordinates of a sufficient number of points of both the true and the apparent orbits, so that the two ellipses may be easily drawn by hand. The co-ordinates have been deduced from Mr. Powell's elements published in the *Monthly Notices*, vol. xxx. page 192, the line of nodes common to both ellipses being taken as the basis. The x co-ordinates are consequently the same for both ellipses, while the (y) co-ordinates of the projected or apparent ellipse have a constant ratio to the y co-ordinates of the true ellipse. The first column of the table contains the corresponding true anomalies v of the comes:—

v °	x "	(y) "	y "	
300·80	+ 8·96	0	0	Node.
330	+ 6·69	+ 3·74	+0·57	
0	+ 3·72	+ 6·23	+0·95	Perihelion.
30·80	0	+ 7·68	+1·17	
49·91	− 2·76	+ 7·96	+1·22	Maximum of y.
70	− 6·17	+ 7·57	+1·15	
90	−10·22	+ 6·09	+0·93	
110	−14·96	+ 2·85	+0·44	
120·80	−17·96	0	0	Node.
129·75	−19·88	− 3·13	−0·48	End of minor axis.
140	−22·03	− 7·67	−1·17	
154·12	−23·41	−15·39	−2·35	Maximum of x.
170	−21·00	−24·33	−3·71	
180	−16·90	−28·35	−4·33	Aphelion.
191·69	−10·42	−30·08	−4·59	Maximum of y.
200	− 5·59	−29·29	−4·47	
210·80	0	−26·40	−4·03	
220	+ 3·73	−23·02	−3·51	
230·25	+ 6·70	−18·98	−2·90	End of minor axis.
240	+ 8·54	−15·27	−2·33	
250	+ 9·63	−11·80	−1·80	
267·48	+10·23	− 6·73	−1·03	Maximum of x.
270	+10·22	− 6·09	−0·93	
290	+ 9·59	− ·	−0·28	
Centre of ellipse	− 6·59	−11·06	−1·69	

The circle of declination passes through the points—

	x = "	y = "
North	+ 20	− 9·03
South	− 20	+ 9·03

The co-ordinates of the chief positions of the comes, which have been deduced from observations, and have been published, are the following (*vide Monthly Notices*, vols. xv. p. 88, xvi. p. 120, xvii. p. 19, xxiv. p. 170, and *Memoirs*, vols. xxiv. xxv. and xxxii.)

Time.	x	y	Observer.	Place.
	"	"		
1826·012	−22·18	−3·47	Dunlop	Paramatta
30·012	−19·60	−3·71	Johnson	St. Helena
34·79	−16·87	−4·27	Herschel	Feldhausen
37·34	−15·46	−4·55	,,	,,
46·866	− 8·50	−4·91	Jacob	Madras
48·023	− 6·70	−4·46	,,	,,
50·926	− 4·12	−4·33	,,	,,
53·049	− 2·05	−4·06	,,	,,
54·003	− 1·30	−4·01	Jacob & Powell	,,
55·318	+ 0·09	−4·07	Powell	,,
56·015	+ 0·54	−3·77	Jacob	,,
56·591	+ 1·11	−3·72	,,	,,
59·382	+ 3·67	−3·57	Powell	,,
60·114	+ 4·41	−3·58	,,	,,
60·479	+ 4·55	−3·26	,,	,,
61·052	+ 5·08	−3·33	,,	,,
61·305	+ 5·31	−3·20	,,	,,
61·576	+ 5·45	−3·15	,,	,,
62·205	+ 6·09	−3·01	,,	,,
63·028	+ 6·63	−2·80	,,	,,
64·110	+ 7·44	−2·50	,,	,,
1870·1	+10·22	−0·69	,,	,,

Predicted Positions.

	x	y
	"	"
1872·0	+ 8·64	+0·11
73·0	+ 6·88	+0·52
74·0	+ 4·31	+0·89
75·0	+ 1·16	+1·13
76·0	− 2·17	+1·21
77·0	− 5·28	+1·18
1878·0	− 8·07	+1·07

The first four of these places have been observed in the Southern Hemisphere, all the others at Madras, where *α Centauri* does not reach an altitude of 17° above the horizon. Powell's

published observations represent upwards of 2,000 single measures (*vide Memoirs*, vol. xxxii. p. 93). Of his later observations made after 1864, only the normal place for 1870–1 seems to have become publicly known. It is to be hoped that he may have been able to watch and observe the comes of his favourite double star through the most interesting portion of its orbit; but with the star so near the horizon and with a 4-inch telescope, the task cannot but have been beset with considerable difficulties. Mr. Powell will have deserved all the more credit and honour. It might fairly have been expected that observers who are provided with better instruments, and are far more favourably placed for observing α *Centauri*, would have rejoiced at having the opportunity of observing it just during those years when the observations are of the highest permanent value. If Mr. Powell's orbit and remarks were not sufficient to attract their attention, a glance over the Ephemeris deduced by Mr. Hind from Powell's elements, and published in the *Monthly Notices* for November 1872, vol. xxx. page 54, might have shown them the lucky and important chance within their grasp. The observations need not have interfered with other work, as they demand no fixed days and hours, and might have been made in daytime. But it seems that the most favourable time and opportunity has been allowed to slip away unused. However, as a couple of position-angles which Lord Lindsay has obtained in 1874 during his Mauritius expedition, are said to indicate that the comes of α *Centauri* is more than half a year behind its predicted position, the Southern observers may have a chance of regaining a portion of their neglected opportunities; and I suggested, therefore, that their personal friends should call their attention to the matter. But as you and other members of the Council have expressed the opinion that it would be better that I should put my remarks on paper for the *Monthly Notices*, I defer to your and their opinion, and I place accordingly these lines at your disposal, in the hope that they may be of some service.

1876, *December* 13.

A Preliminary List of Binary and other interesting Double Stars.

By J. M. Wilson, Esq., M.A., and J. Gledhill, Esq., F.R.A.S.

The following list is intended to be the basis of a working List of Binary and other interesting Double Stars, of which it is desirable to have accurate measures at not very distant intervals of time; and we think it may be of assistance to those observers who take up this branch of astronomy to have such a list for their guidance. We publish it also, with the hope that it may attract the attention of those who have specially devoted themselves to the measurement of binary stars, and that they will by their criticisms point out any errors that may exist in it, of

omission or otherwise. It is our intention to offer to the Society, at no distant period a list of binaries and other interesting double stars, with all the measures of them, that have been published up to the present date, extracted from the various Memoirs, Transactions, Journals, etc., which an astronomer must now consult if he wishes to ascertain the past history of a binary star and compute its orbit. It is plainly desirable that this list should be as complete as possible, and we shall be greatly obliged to any one who will point out to either of us any omissions in this preliminary list.

With a view to this work, we formed, some years ago, a list of double stars for observation, selected from those observed by Σ, OΣ, Mädler, Dawes, Dembowski, Secchi, and others, and have measured them, in order to ascertain, in all doubtful cases, whether there was sufficient evidence of appreciable change having taken place since the earliest measures. In this way we have been enabled to strike off many that were classed as probable binaries by Mädler and Secchi, and have ascertained that others are in motion which were not previously given in lists of such objects. Clearly, however, no list published now, however perfect, can possibly be final; it can do no more than represent knowledge up to the present date.

In addition to the well-known Lists of Double-Star Measures, we have been enabled, at the request of the Rev. R. Main, to obtain from Herr Otto von Struve the yet unpublished Part I. of his forthcoming great work on Double Stars. The Part received contains the Pulkova measures of Σ's Double Stars. Part II., containing the re-examination of OΣ's own discoveries, is expected shortly. We have found this work of very great help in deciding several doubtful points.

The numerous close pairs discovered by Mr. S. W. Burnham, of which measures have been published, will be entered on this list. There can be but little doubt that these interesting and difficult stars will shortly yield a rich harvest of binaries.

A few words on the form of the list may here be given. The first column contains the reference number; the second, the name of the star; the third, Σ's or OΣ's number (the latter in brackets) where the star has been observed by either; the fourth, the number in Sir John Herschel's great Catalogue, lately edited by the Rev. R. Main and Prof. Pritchard.

In conclusion, the points on which we request information are—

(1) Binaries omitted which it is thought should appear in such a list.

(2) Criticisms on the insertion of any stars which it is thought should be omitted.

(3) Unpublished measures or orbits of any binary stars.

Communications may be sent to J. M. Wilson, Temple Observatory, Rugby; or to Mr. J. Gledhill, Mr. E. Crossley's Observatory, Halifax.

No.	Name of Star.	Σ's No.	H's No.
1	Cephei 316 (B)	2	2
2	... ...	[2]	35
3	Cephei 318 (B)	13	48
4	... ...	16	58
5	... ...	[4]	59
6	... ...	23	66
7	26 Andromedæ	[5]	76
8	42 Piscium ...	27	103
9	Cassiopeiæ 49 (B)	30	127
10	49 Piscium ...	32	156
11	λ Cassiopeiæ ...	[12]	162
12	... ...	[18]	242
13	η Cassiopeiæ ...	60	283
14	... ...	69	307
15	36 Andromedæ...	73	319
16	P. O. 251 ...	80	344
17	... ...	86	373
18	Ceti 160 (B) ...	91	393
19	Polaris ...	93	400
20	... ...	[24]	407
21	... ...	[28]	430
22	... ...	102	453
23	42 Ceti ...	113	474
24	... ...	118	502
25	... ...	122	514
26	... ...	125	515
27	... ...	132	542
28	Andromedæ 219 (B)	133	543
29	P. I. 123 ...	138	568
30	... ...	142	588
31	... ...	[35]	606
32	... ...	158	637
33	... ...	175	677
34	... ...	183	704
35	... ...	185	710
36	P. I. 209 ...	186	714
37	... ...	196	738
38	α Piscium ...	202	753
39	γ Andromedæ ...	[38]	755

No.	Name of Star.	Σ's No.	H's No.
40	10 Arietis ...	208	761
41	... ...	221	799
42	Andromedæ 259 (B)	228	818
43	66 Ceti ...	231	821
44	... ...	234	827
45	... ...	257	892
46	ι Cassiopeiæ ...	262	906
47	... ...	278	949
48	84 Ceti ...	295	1009
49	θ Persei ...	296	1010
50	γ Ceti ...	299	1019
51	Arietis 114 (B)	305	1036
52	... ...	312	1044
53	π Arietis ...	311	1047
54	Persei 85 (B) ...	314	1053
55	... ...	[48]	1058
56	... ...	326	1080
57	... ...	328	1084
58	ε Arietis ...	333	1098
59	... ...	334	1104
60	Persei 104 (B)...	336	1109
61	Procyon		
62	... ...	[50]	1132
63	... ...	355	1147
64	... ...	367	1179
65	... ...	377	1210
66	... ...	380	1222
67	... ...	388	1240
68	... ...	400	1270
69	... ...	403	1271
70	... ...	408	1279
71	7 Tauri ...	412	1288
72	P. III. 98 ...	422	1308
73	... ...	447	1370
74	P. III. 165 ...	[64]	1387
75	Cephei 49 (Hev.)	460	1406
76	32 Eridani ...	470	1436
77	P. III. 242 ...	[531]	1486
78	... ...	511	1528

No.	Name of Star.	Σ's No.	H's No.
79	... ...	[78]	1540
80	40 Eridani ...	518	1553
81	... ...	[79]	1571
82	... ...	[80]	1582
83	Tauri 230 (B) ...	535	1600
84	... ...	[82]	1602
85	... ...	[85]	1677
86	2 Camelopardali	566	1687
87	... ...	567	1690
88	... ...	577	1715
89	... ...	615	1831
90	... ...	619	1842
91	5 Aurigæ	[92]	1844
92	... ...	[93]	1862
93	... ...	629	1871
94	Camelopardali 19 (Hev.)	634	1892
95	P. IV. 288 ...	[95]	1897
96	... ...	[97]	1903
97	14 Orionis (*i*) ...	[98]	1923
98	... ...	644	1925
99	... ...	[100]	1941
100	... ...	651	1947
101	λ Aurigæ ...	...	1991
102	... ...	676	2001
103	... ...	677	2005
104	η Orionis ...	...	2071
105	... ...	712	2091
106	32 Orionis ...	728	2133
107	Tauri 380 (B) ...	742	2165
108	θ^1 Orionis ...	748	2178
109	... ...	749	2182
110	... ...	[112]	2190
111	ζ Orionis ...	774	2235
112	... ...	3115	2237
113	... ...	853	2462
114	... ...	861	2475
115	4 Lyncis ...	881	2527
116	... ...	932	2695
117	... ...	945	2730

No.	Name of Star.	Σ's No.	H's No.
118	54 Aurigæ ...	[152]	2734
119	12 Lyncis ...	948	2749
120	... ...	3117	2757
121	... ...	[154]	2766
122	... ...	[155]	2785
123	... ...	[156]	2795
124	Sirius ...	...	2799
125	14 Lyncis ...	963	2802
126	... ...	[157]	2811
127	13 Lyncis ...	[159]	2851
128	... ...	[161]	2869
129	38 Geminorum (ϵ)	982	2872
130	μ Canis Majoris	997	2899
131	Σ 1037 ...	[166]	3012
132	... ...	1049	3040
133	P. VII. 52 ...	[170]	3068
134	δ Geminorum ...	1066	3084
135	... ...	1071	3092
136	... ...	1074	3103
137	... ...	1076	3107
138	... ...	1081	3121
139	... ...	1091	3158
140	... ...	1093	3161
141	... ...	1104	3214
142	Castor ...	1110	3228
143	P. VII. 170 ...	1126	3297
144	κ Geminorum ...	[179]	3321
145	... ...	1136	3340
146	... ...	1142	3354
147	... ...	1157	3420
148	... ...	[186]	3482
149	Lyncis 85 (B) ...	1187	3533
150	ζ Cancri ...	1196	3557
151	P. VIII. 13 ...	1202	3572
152	... ...	1216	3646
153	... ...	[193]	3696
154	... ...	1263	3832
155	ϵ Hydræ ...	1273	3868
156	... ...	1287	3907

No.	Name of Star.	Σ's No.	H's No.
157	ι Ursæ Majoris	[196]	3943
158	... ...	1300	3970
159	σ^2 Ursæ Majoris	1306	3989
160	... ...	1316	4021
161	... ...	1321	4046
162	... ...	3121	4083
163	Lyncis 157 (B)...	1338	4101
164	... ...	[200]	4123
165	... ...	[201]	4128
166	Hydræ 116 (B)...	1348	4139
167	ω Leonis ...	1356	4165
168	Hydræ 134 (B)...	1365	4190
169	P. IX. 161 ...	1377	4253
170	φ Ursæ Majoris	[208]	4290
171	... ...	1385	4294
172	... ...	1389	4305
173	8 Sextantis ...	A.C. 5	4314
174	... ...	1406	4387
175	... ...	[213]	4429
176	P. X. 23 ...	[215]	4449
177	γ Leonis ...	1424	4469
178	Leonis 145 (B)...	1426	4477
179	... ...	1439	4536
180	... ...	1457	4606
181	P. X. 128 ...	[224]	4612
182	... ...	1472	4669
183	... ...	[228]	4671
184	... ...	[229]	4690
185	... ...	1486	4714
186	... ...	[230]	4717
187	54 Leonis ...	1487	4719
188	... ...	1500	4754
189	P. X. 229 ...	1504	4782
190	... ...	1514	4820
191	[539] ...	1516	4833
192	P. XI. 9 ...	1517	4834
193	ξ Ursæ Majoris	1523	4860
194	... ...	1534	4885
195	ι Leonis ...	1536	4896

No.	Name of Star.	Σ's No.	H's No.
196	57 Ursæ Majoris	1543	4924
197	... ...	[234]	4934
198	... ...	[235]	4942
199	P. XI. 111 ...	1555	4978
200	... ...	[237]	5000
201	... ...	1588	5141
202	... ...	3123	5167
203	... ...	1607	5205
204	Comæ Ber. 68 (B)	1639	5293
205	... ...	1641	5296
206	... ...	1644	5307
207	Virginis 191 (B)	1647	5319
208	... ...	1658	5341
209	... ...	1663	5354
210	γ Virginis ...	1670	5377
211	... ...	1678	5401
212	35 Comæ Ber. ...	1687	5430
213	... ...	[256]	5445
214	179 Comæ Ber.	1722	5515
215	42 Comæ Ber. ...	1728	5523
216	... ...	[261]	5535
217	... ...	1734	5570
218	... ...	1742	5590
219	... ...	1746	5608
220	72 Virginis l^1 ...	1750	5622
221	P. XIII. 127 ...	1757	5639
222	25 Canum Ven.	1768	5673
223	84 Virginis ...	1777	5704
224	... ...	1781	5726
225	τ Boötis ...	[270]	5737
226	... ...	1785	5754
227	P. XIII. 238 ...	1788	5789
228	[277] ...	1812	5894
229	... ...	1819	5907
230	... ...	1820	5913
231	Boötis 121 (B)...	1825	5922
232	... ...	1830	5933
233	... ...	1832	5934
234	P. XIV. 70 ...	1837	5964

No.	Name of Star.	Σ's No.	H's No.
235	... ...	1842	5987
236	... ...	1858	6040
237	... ...	1863	6062
238	π Boötis ...	1864	6066
239	ζ Boötis ...	1865	6069
240	... ...	1866	6072
241	... ...	1876	6099
242	ϵ Boötis ...	1877	6101
243	... ...	1879	6106
244	... ...	1883	6124
245	ξ Boötis ...	1888	6146
246	... ...	[287]	6159
247	... ...	[288]	6161
248	44 Boötis (*i*) ...	1909	6237
249	... ...	1925	6305
250	... ...	[295]	6311
251	5 Serpentis ...	1930	6327
252	Coronæ 1 (B) ...	1932	6331
253	... ...	1934	6336
254	η Coronæ ...	1937	6362
255	P. XV. 74 ...	1938	6371
256	... ...	1944	6382
257	... ...	[296]	6388
258	δ Serpentis ...	1954	6426
259	... ...	1957	6434
260	... ...	[298]	6446
261	... ...	1961	6440
262	γ Coronæ ...	1967	6469
263	... ...	1983	6523
264	... ...	1985	6535
265	18 π^2 Ursæ Minoris	1989	6547
266	... ...	[303]	6575
267	ξ Libræ ...	1998	6582
268	κ Herculis ...	2010	6610
269	49 Serpentis ...	2021	6634
270	... ...	2022	6640
271	... ...	2026	6645
272	σ Coronæ ...	2032	6654
273	Antares ...	...	6707

No.	Name of Star.	Σ's No.	H's No.
274	... ...	2049	6718
275	Draconis 99 (B)	2054	6723
276	λ Ophiuchi ...	2055	6727
277	ζ Herculis ...	2084	6799
278	... ...	2094	6816
279	... ...	2097	6823
280	21 Ophiuchi ...	[315]	6840
281	... ...	2106	6842
282	Herculis 167 (B)	2107	6847
283	... ...	3107	6867
284	... ...	[321]	6879
285	P. XVI. 270 ...	2114	6888
286	20 Draconis ...	2118	6895
287	Herculis 210 (B)	2120	6910
288	μ Draconis ...	2130	6935
289	36 Ophiuchi ...	...	6946
290	δ Herculis ...	3127	6968
291	... ...	2145	6973
292	ρ Herculis ...	2161	7016
293	... ...	2165	7028
294	... ...	2173	7040
295	P. XVII. 163 ...	2190	7076
296	... ...	2199	7104
297	... ...	2203	7108
298	... ...	2205	7128
299	... ...	2214	7129
300	... ...	2215	7130
301	μ Herculis (A. C. 7)	2220	7142
302	τ Ophiuchi ...	2262	7245
303	... ...	2267	7262
304	... ...	2271	7267
305	70 *p* Ophiuchi ...	2272	7273
306	73 Ophiuchi ...	2281	7309
307	Herculis 417 (B)	2289	7322
308	... ...	2294	7340
309	Lalande 33731 ...	2303	7370
310	... ...	2311	7388
311	Herculis 452 (B)	2315	7406
312	39 Draconis, *b* ..	2323	7425

No.	Name of Star.	Σ's No.	H's No.
313	ϕ Draconis ...	[353]	7443
314	... ...	[358]	7479
315	α Lyræ ...	...	7501
316	... ...	2367	7523
317	... ...	2384	7563
318	ϵ^1 Lyræ ...	2382	7564
319	ϵ^2 Lyræ ...	2383	7566
320	... ...	2396	7593
321	... ...	2400	7604
322	... ...	2402	7609
323	... ...	2409	7625
324	o Draconis ...	2420	7660
325	[365] ...	3130	7670
326	... ...	2422	7671
327	11 Aquilæ ...	2424	7675
328	... ...	2429	7689
329	P. XVIII. 274...	2434	7702
330	... ...	2437	7706
331	P. XVIII. 287...	2438	7709
332	... ...	2441	7723
333	... ...	2454	7752
334	Lalande 35821...	2455	7753
335	... ...	2464	7768
336	... ...	2471	7787
337	... ...	2481	7810
338	... ...	2484	7819
339	Cygni 6 ...	2486	7828
340	... ...	2491	7854
341	... ...	2514	7922
342	P. XIX. 128 ...	2521	7946
343	Cygni 22 (B) ...	2525	7958
344	... ...	2538	8006
345	P. XIX. 185 ...	2541	8024
346	... ...	2544	8037
347	... ...	[378]	8061
348	... ...	2556	8079
349	... ...	[383]	8123
350	... ...	2574	8139

No.	Name of Star.	Σ's No.	H's No.
351	... ...	2576	8146
352	δ Cygni ...	2579	8153
353	... ...	[387]	8179
354	β Aquilæ ...	[532]	8228
355	Cygni 116 (B) [392]	2607	8274
356	... ...	2619	8313
357	θ Sagittæ ...	2637	8382
358	... ...	2640	8386
359	... ...	[400]	8411
360	... ...	2658	8457
361	P. XX. 178 [407]	2690	8600
362	... ...	2696	8624
363	β Delphini ...	2704	8663
364	κ Delphini ...	[533]	8674
365	... ...	2708	8692
366	... ...	[410]	8703
367	... ...	2725	8751
368	52 Cygni ...	2726	8755
369	λ Cygni ...	[413]	8773
370	4 Aquarii ...	2729	8784
371	... ...	2734	8812
372	ε Equulei ...	2737	8839
373	... ...	2744	8860
374	... ...	2746	8868
375	... ...	2749	8876
376	61 Cygni ...	2758	8898
377	... ...	2760	8902
378	δ Equulei [535]	2777	8959
379	... ...	2779	8965
380	P. XXI. 50 ...	[432]	8976
381	A. C. 19 ...	...	8998
382	... ...	2793	9048
383	... ...	[437]	9021
384	Pegasi 20 (B) ...	2799	9072
385	... ...	2801	9087
386	Pegasi 29 (B) ...	2804	9107
387	... ...	[443]	9134
388	... ...	[447]	9175

No.	Name of Star.	Σ's No.	H's No.
389	μ Cygni ...	2822	9210
390	... ...	2825	9226
391	... ...	2828	9240
392	... ...	2837	9273
393	... ...	2849	9333
394	... ...	2865	9416
395	P. XX. 11, 12 ...	2872	9442
396	P. XXII. 33 ...	2877	9469
397	Pegasi 148 ...	2878	9466
398	... ...	2895	9516
399	33 Pegasi ...	2900	9539
400	ζ Aquarii ...	2909	9580
401	37 Pegasi ...	2912	9593
402	... ...	2915	9614
403	... ...	2928	9670
404	... ...	2934	9703
405	... ...	2942	9736
406	P. XXII. 219 ...	2944	9742
407	... ...	[536]	9832
408	52 Pegasi ...	[483]	9840
409	... ...	2976	9901
410	... ...	2990	9946
411	94 Aquarii ...	2998	9982
412	*o* Cephei ...	3001	9993
413	... ...	3006	10004
414	... ...	3007	10015
415	P. XXIII. 69 ...	3008	10020
416	... ...	[500]	10117
417	... ...	3037	10170
418	... ...	3046	10235
419	37 Andromedæ (B)	3050	10258
420	Lalande 47206...	3056	10291
421	... ...	3062	10304
422	... ...	[523]	

The following stars, having common proper motion (see Struve's *Pos. Med.*) may, perhaps, be measured occasionally with advantage, and should therefore be inserted in the list. The stances are between $32''$ and $7'$: 40 *Eridani*, ψ *Aquarii*, 37 *Ceti*,

Regulus, 16 *Cygni*, *Castor*, ν^1, ν^2 *Draconis*, 1 *Pegasi*, ι *Boötis*, 93 *Leonis*, μ *Boötis*, θ^1, θ^2 *Tauri*, ε and 5 *Lyræ*, α *Tauri*, 6 and 8 *Vulpec.*, 16 and 17 *Draconis*, 33 and 34 *Ophiuchi*, α^1 and α^2 *Capricorni*; also P. III. 241 and 50 *Persei* (see O Σ and Argelander).

In addition to the above, a good many pairs, whose changes in angle and distance can be explained by the proper motion of the principal star, were long ago examined by Struve (*Pos. Med.*) Perhaps these should go into the list. Also the "higher systems" of Mädler would probably not be misplaced in it (see *Untersuchungen über die Fixstern-Systeme.*

Mr. E. Crossley's Observatory,
Bermerside, Halifax,
December 1876.

On an Oversight in the Mécanique Céleste, and on the Internal Densities of the Planets. By George H. Darwin, M.A., Fellow of Trinity College, Cambridge.

(*Communicated by J. W. L. Glaisher, F.R.S.*)

In the following paper an endeavour is made to point out an inconsistency, which appears to have escaped the notice of Laplace, in his determination of the precessional constants of the planets *Jupiter* and *Saturn*. From this I have been led on to speculate on the law of internal density of those planets, and of *Mars*, and to make some reference to the ellipticities of *Mercury* and *Venus*.

1. *Laplace's Law of the Internal Density of the Planets.*

In the investigation of the figure of the Earth, Laplace assumed that, in molten rock, the hydrostatic pressure *plus* a constant varies as the square of the density. The result of this assumption is that, after the consolidation of a planet, the density of any stratum of mean radius r is given by the law $\frac{\mathrm{F}}{r}\sin\frac{r\theta}{a}$, where a is the mean radius of the surface, and θ and F are constants.

Throughout the rest of this paper, besides the foregoing, the following notation is used:—

a, β, the equatoreal and polar radii;
ε the ellipticity of the surface;
m the ratio of the centrifugal force of the planet's rotation at the distance a to the mean pure gravity;

f the ratio of the mean to the surface density;
C, A, the greatest and least principal moments of inertia of the planet;
P or $\frac{C-A}{C}$ the precessional constant; and
M the mass.
Other symbols will be defined as they arise.

Then the results of the Laplacian law may be embodied in the following equations*:—

$$\frac{f}{3} = \frac{1}{\theta^2} - \frac{\cot\theta}{\theta} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

$$\frac{5m}{2\epsilon} = \frac{f\theta^2}{3(f-1)} - \frac{3}{f} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

$$C-A = \frac{2}{3}\left(\epsilon - \frac{m}{2}\right) Ma^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

$$C = \frac{2}{3}\left\{1 - \frac{6(f-1)}{f\theta^2}\right\} Ma^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

$$\frac{\epsilon - \frac{m}{2}}{P} = 1 - \frac{6(f-1)}{f\theta^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

Now, if (following Thomson and Tait) we take for the Earth $f = 2{\cdot}1$, which corresponds with $\theta = 144°$, we have

$$P = 1{\cdot}994\left(\epsilon - \frac{m}{2}\right) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

$$\frac{5m}{2\epsilon} = 2{\cdot}562 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

Hence, in any planet where the law of density is the same as in the Earth (i.e., $f = 2{\cdot}1$, $\theta = 144°$), we should find (7) satisfied, and (6) would give the precessional constant.

In order to illustrate the Laplacian law, the following table has been constructed, giving the values of f and $\frac{5m}{2\epsilon}$ for every 10° of θ, with the omission of a few at the early part, which are rather troublesome to calculate, and are of little value.

* See Thomson and Tait, *Nat. Phil.* §§ 824, 827. Equation (3) is independent of Laplace's assumption.

θ	f	$\frac{5m}{2\varepsilon}$	θ	f	$\frac{5m}{2\varepsilon}$
0°	1·0000	2·000	90°	1·2159	2·165
10			100	1·2879	2·213
20	1·0082		110	1·3827	2·270
30	1·0188		120	1.5109	2·338
40	1·0341	2·029	130	1·6922	2·422
50	1·0548	2·046	140	1·9657	2·525
60	1·0817	2·067	150	2·4225	2·652
70	1·1161	2·094	160	3·3363	2·813
80	1·1600	2·126	170	6·0750	3·019
90	1·2159	2·165	180	infin.	3·290

This table shows how $\frac{5m}{2\varepsilon}$ increases, as the planet passes from homogeneity to infinitely small surface density; $\frac{5m}{2\varepsilon}$ can never be less than 2, nor greater than 3·290. Although it is not strictly involved in the subject of this paper, I may point out that the first column of the table may be employed to calculate the ellipticity of any internal stratum. I have shown elsewhere* that the ellipticity e of any stratum of mean radius $\frac{\theta'}{\theta}a$ is given by,—

$$\frac{e}{\varepsilon} = \left(\frac{\theta}{\theta'}\right)^2 \frac{f(f'-1)}{f'(f-1)},$$

where f, θ refer to the surface, and f' is the value corresponding to θ' in the table. For example, to find the ellipticity of the Earth's stratum of mean radius $\frac{5}{12}a$, we must take $\theta' = 60°$, because $\theta = 144°$; then from the table $f' = 1·0817$, and

$$e = \varepsilon \left(\frac{12}{5}\right)^2 \cdot \frac{2·1 \times ·0817}{1·0817 \times 1·1} = \frac{\varepsilon}{1·204}$$

$$= \frac{1}{357}, \text{ when } \varepsilon = \frac{1}{297}.$$

2. *Jupiter's Precessional Constant.*

To determine the precessional constant Laplace uses the following argument:—"If we suppose the densities of the strata of Jupiter and the Earth, at distances proportional to the diameters of these planets, to be in a constant ratio to each other. . . In this hypothesis, if both planets be fluid, their ellipticities will be,

* *Messenger of Mathem.*, Nov. 1876, p. 109. The notation is, however, different.

as in [2068 *k*, etc., III. v. § 43] proportional to the respective values of ϕ,* corresponding to each of them; or to their ellipticities, if they be homogeneous. If we suppose the ratio to obtain in their actual state, and we have seen in [2069, III. v. § 43] that this is nearly conformable to observation, *then the values of* $\frac{2C-A-B}{C}$ *will be, for each of these planets, respectively proportional to the ellipticities corresponding to the case of homogeneity.* These ellipticities by the same article [2068″], are as ·10967000 to 0·00433441." † He uses ·00291193 as the Earth's precessional constant, and thence deduces $\frac{2C-A-B}{C} = ·14735$, or in my notation P = ·07368.

By the theory of the perturbations of the Satellites, he finds $\epsilon - \frac{m}{2} = ·0219013$; ‡ this value does involve any assumption as to the law of internal density, except, I conceive, that the strata of equal density must be nearly spherical.§

Then by the periodic time of the Fourth Satellite, and its observed distance from *Jupiter*, he finds $m = ·0987990$; whence $\epsilon = ·0713008$.‡

Using these values of m and ϵ, we get $\frac{5m}{2\epsilon} = 3·4642$. Now Laplace does not seem to have noticed that these values are not only incompatible with the identity of the law of internal density in the *Earth* and *Jupiter*, but are also incompatible with that form of law at all; for, as appears from the table, $\frac{5m}{2\epsilon}$ can never be greater than 3·290.

My attention was first drawn to the point by observing that, if there were this identity of law of density, the precessional constant of *Jupiter* ought to be nearly twice $\epsilon - \frac{m}{2}$, or ·0438; whereas Laplace, by a different method, founded on the same assumption, finds it to be ·0737. This of course indicated that the assumption was untenable.

In view of this discrepancy, it will be well to go over Laplace's work again by the light of later, and presumably better observations.

The distance of the Fourth Satellite is open to some doubt, but the following has seemed to me the best value attainable. Bessel gives the apparent distance at the planet's mean distance the angles as 498·8663″.**

* The m of this paper.
† Bowditch, Trans. of *Méc. Cél.* VIII. vii., § 23.
‡ VIII. ix. § 27.
§ See Pratt's *Fig. of Earth*, Arts. 90–94.
** *Astron. Untersuch. Bestim. der Masse des Jupiter*, Königsberg 1842.

M. Kaiser gives, as the result of a long series of his own observations when united with those of Bessel, that the polar and equatoreal diameters of *Jupiter* subtend at the same distance 35·170″ and 37·563″ respectively.*

Hence the mean distance D of the Satellite is 26·5616 equatoreal radii; or D = 26·5616 a; T, the satellite's periodic time is 16·68902 m. s. days, t, *Jupiter's* sidereal day is ·4135 m. s. days. Therefore

$$m = \left(\frac{T}{t}\right)^2 \left(\frac{a}{D}\right)^3 (1-\epsilon)$$

$$= \cdot 0869258\,(1-\epsilon) = m'\,(1-\epsilon) \text{ suppose.}$$

Professor J. C. Adams informs me that M. Damoiseau has recalculated the values of $\epsilon - \frac{m}{2}$, and of the masses of the Satellites, basing his work on better observations than those which were available to Laplace.† He has taken, however, certain coefficients from Laplace, which ought to have been recalculated. This work has been completed by Professor Adams himself, and he has obtained the following value for $\epsilon - \frac{m}{2}$, of which he has, with very great kindness, allowed me to make use, viz.,

$$\epsilon - \frac{m}{2} = \cdot 0216623‡$$

Substituting then for m, we have, for the ellipticity of *Jupiter*,—

$$\epsilon = \frac{\frac{m'}{2} + \cdot 0216623}{1 + \frac{m'}{2}} = \cdot 06241 = \frac{1}{16\cdot 022},$$

and

$$m = m'\,(1-\epsilon) = \cdot 081501.§$$

Laplace remarks** that the ellipticity of *Jupiter* may be found

* *Astron. Nachr.* 48, p. 111.

† *Tables des Satellites de Jupiter.* Bur. des Long., Bachelier, Paris 1836.

‡ The corresponding masses of the Satellites multiplied by 1000 are found by him to be:

·283113
·232355
·812453
·214880

They seem to bear hardly any relation to the ordinarily received values.

§ This value may be compared with ·08163 found by Plana, *Ast. Nach.* 36, p. 155.

** VIII. ix. § 27.

by this method with greater accuracy than by the best observations. It is interesting, therefore, to observe how closely this value agrees with that of all the later observations. The following are the values given for the reciprocal of the ellipticity by—Secchi, 16·06 [*Ast. Nach.* 43, p. 142]; Kaiser, (i) 15·36, (ii) 15·98 [*Ast. Nach.* 45, p. 211]; Bessel and Kaiser, 15·70; Bessel, 15·73; Secchi, 15·99 [*Ast. Nach.* 48, p. 111]; Schmidt, 15·6 [*Ast. Nach.* 65, p. 102]; Main, 16·84 [*Month. Not. Ast. Soc.* 16, p. 142]. The reductions were in several cases made by myself.

It is, nevertheless, a curious coincidence that the value of ϵ found by Laplace agreed well with the older observations of Struve, who found $\epsilon = \frac{1}{13{\cdot}74}$. [*Ast. Nach.* 45, p. 211.]

Using the above value of m, I find (following Plana*) that on the hypothesis of homogeneity $\frac{\alpha}{\beta} = 1{\cdot}1164$ (compare with 1·116515 of Plana), and therefore the homogeneous ellipticity is ·10405. But the homogeneous ellipticity of the Earth is $\frac{1}{230{\cdot}433}$ (Plana); and the Earth's precessional constant is ·003272; hence, following Laplace's method, we should have for *Jupiter*,

$$\begin{aligned} P &= {\cdot}10405 \times 230{\cdot}433 \times {\cdot}003272 \\ &= {\cdot}078451. \end{aligned}$$

But if Laplace's assumption as to the internal density of *Jupiter* were justifiable, we ought to have by equation (6),

$$\begin{aligned} P &= 1{\cdot}994\left(\epsilon - \frac{m}{2}\right) = 1{\cdot}994 \times {\cdot}02166 \\ &= {\cdot}043. \end{aligned}$$

Thus these new values of the quantities involved leave as wide a discrepancy as before between the two values of P. Therefore, Laplace's method cannot be justified.

3. *The Internal Density of Jupiter.*

But this unjustifiability may be seen in another way, for the new values of m and ϵ give $\frac{5m}{2\epsilon} = 3{\cdot}2646$; and reference to equation (7) shows that this differs widely from the corresponding value for the Earth. It is not, however, inconsistent with the Laplacian form of law, as was the same function when Laplace's values of m and ϵ were used. In fact, I find, after some rather troublesome arithmetic, that $\frac{5m}{2\epsilon} = 3{\cdot}2646$ corresponds to $\theta = 179^\circ\ 11'\ 20''$ nearly, and to $f = 68$.

* All future references to M. Plana are to his paper in *Astr. Nachr.* 36, p. 154.

Using these values of θ and f, I find by equation (5) that *Jupiter's* precessional constant $= 2{\cdot}528\left(\epsilon - \frac{m}{2}\right) = {\cdot}0548$; and this I take to be far nearer the truth than the value assigned by Laplace.

On account of the uncertainty in the determination of the quantity m, the above values of θ and f can, of course, have no claim to precision. In fact, if following Herschel, Loomis, and others, we take $\frac{D}{a} = 26{\cdot}99835$, we should find $\epsilon = \frac{1}{16{\cdot}52}$, $m = {\cdot}0778$, and $\frac{5m}{2\epsilon} = 3{\cdot}211$. This last corresponds to θ about $177^\circ\ 42'$, and f about 24.

The value of the precessional constant will, however, be but little changed, being $2{\cdot}489\left(\epsilon - \frac{m}{2}\right) = {\cdot}0539$.

Now it seems reasonable *à priori* to assume that the law of internal density within *Jupiter* is of the same *nature* as in the Earth, and from that assumption it follows that the surface density of *Jupiter* is vastly less than the mean: for it must, I think, be admitted that numerical values can be assigned to m and ϵ with some precision.

But the true meaning of this result would seem to be that the Laplacian law of density is not exact for *Jupiter*, but that that planet must be very much denser in the centre than at the surface. Is it not possible that *Jupiter* may still be in a semi-nebulous condition, and may consist of a dense central part with no well-defined bounding surface? Does not this view accord with the remarkable cloudy appearance of the disk, and the remarkable belts?

4. *Saturn.*

The preceding method cannot be applied very satisfactorily to the case of *Saturn*, on account of the uncertainty in all the quantities which enter into the determination of m and ϵ; but the balance of evidence appears to me decidedly in favour of his surface density being far less than the mean.

m may be best determined from the motion of *Japetus*.

There seems almost complete unanimity as to his periodic time, and I take $T = 79{\cdot}3296$ m. s. days.

Two values are assigned for the sidereal day of *Saturn*, viz., ·4370 (Hansen), and ·4278 (Herschel) m. s. days, the latter being more generally accepted; I take then $t = {\cdot}430$.

There is a wide difference of opinion as to the mean distance D of *Japetus*, the estimates varying from $57{\cdot}4\,a$ (Jacob, *Mem. Astr. Soc.* vol. xxviii.) to $64{\cdot}359a$ (used by Herschel, Loomis, and others, and most generally accepted). I take $D = 62\,a$.

The ellipticity seems fairly well determined at $\frac{1}{11}$.*

Using these quantities in the formula

$$m = \left(\frac{T}{t}\right)^2 \left(\frac{a}{D}\right)^3 (1-\epsilon),$$

I find

$$m = \cdot 131270 - \cdot 61056\ \delta t - \cdot 00635\ \delta\left(\frac{D}{a}\right) - \cdot 14281\ \delta\epsilon.$$

(i) If all the small variations are zero, and $\epsilon = \frac{1}{11}$, we get $\frac{5m}{2\epsilon} = 3\cdot 610$.

(ii) Loomis, *Pract. Astron.*, Tables 33, 34.

$$t = \cdot 437, \quad \frac{D}{a} = 64\cdot 359, \quad \epsilon = \frac{1}{10},$$

$$\delta t = \cdot 007, \quad \delta\frac{D}{a} = 2\cdot 359, \quad \delta\epsilon = \cdot 01909,$$

then, $m = \cdot 1093$, $\frac{5m}{2\epsilon} = 2\cdot 732$.

(iii) Hind's *Solar System*, pp. 103–4.

$$t = \cdot 4278, \quad \frac{D}{a} = 60\cdot 3436, \quad \epsilon = \frac{1}{10\cdot 28} \text{ according to Bessel},$$

$$\delta t = -\cdot 0022, \quad \delta\frac{D}{a} = -1\cdot 6564, \quad \delta\epsilon = \cdot 00637,$$

then, $m = \cdot 1422$, $\frac{5m}{2\epsilon} = 3\cdot 66$.

If we take $\epsilon = \frac{1}{9\cdot 23}$ from the *Greenwich Observations*, $\frac{5m}{2\epsilon}$ will be slightly smaller.

(iv) Captain Jacob's *Madras Observations* give $\frac{D}{a} = 57\cdot 4$, and $\delta\frac{D}{a} = -4\cdot 6$, this with $\epsilon = \frac{1}{11}$ and all the other small variations zero, gives $m = \cdot 160$ and $\frac{5m}{2\epsilon}$ very nearly $4\frac{1}{4}$.

No doubt other values might be assigned to $\frac{5m}{2\epsilon}$ with equal

* I find the following in a paper by Mr. Grant (*Month. Not. Ast. Soc.* xiii. p. 195), where he reduces observations of apparent ellipticity to their real values, viz., Lassell $\frac{1}{11\cdot 05}$, Main $\frac{1}{9\cdot 227}$, De La Rue $\frac{1}{10\cdot 611}$. The mean of all Bessel's observations would seem to be $\frac{1}{10\cdot 20}$, but Mr. Hind (see below) assigns a slightly different value as Bessel's result.

Sir W. Herschel found $\frac{1}{10\cdot 368}$ (see p. 79 of above vol.), but $\frac{1}{10\cdot 384}$ is also attributed to him on the same page.

plausibility, but I think (ii) and (iv) contain extreme values in the two directions, and the truth probably lies at some intermediate point. It is noticeable that even the value (ii) indicates a larger ratio of mean to surface density than in the Earth, namely 2·809 (corresponding to $\theta = 155^\circ\ 14'$), whilst all the other values are far outside the limits of admissibility under the Laplacian law.

I have tried also to find m from the motion of *Titan*. Taking

$$\frac{D}{a} = 21,\quad T = 15{\cdot}9454,\quad t = {\cdot}43,\quad \epsilon = \frac{1}{11},$$

I find

$$m = {\cdot}13648 - {\cdot}1174\,\delta t - {\cdot}0195\,\delta\frac{D}{a} - {\cdot}1485\,\delta\epsilon.$$

If the small increments are zero, $\frac{5m}{2\epsilon} = 3{\cdot}75$; and if the various possible values be assigned to them, the results will be found to be very similar to those given above.

The value of m assigned by Laplace is ·165970,* and this, together with the ellipticities $\frac{1}{9}$ and $\frac{1}{12}$, gives $\frac{5m}{2\epsilon}$ equal to 3·734 and 4·979 respectively, either value quite incompatible with the Laplacian law of density. The ellipticity of the planet had apparently not been observed in his time, and the numbers used by him involve an ellipticity of $\frac{1}{68{\cdot}6}$, which we now know is far too small.

Laplace makes *Saturn's* precessional constant $\frac{1}{2}$ of ·27934 or ·13967 (VIII. xvii. § 37), but this value is inconsistent with any of those given above for m and e.

Taking $\epsilon = \frac{1}{11}$, $m = {\cdot}1313$, we have $\epsilon - \frac{m}{2} = {\cdot}0252$. It can hardly be supposed that *Saturn* is more nearly homogeneous than is the Earth, and if the law of density were the same we should, by (6), have P = ·050.

On the other hand, if the Laplacian law of density were to hold good, and if the surface density were zero—of course an ideal case†—we should have by (5),

$$\frac{\epsilon - \frac{m}{2}}{P} = 1 - \frac{6}{\pi^2} = \frac{1}{2{\cdot}5505},$$

or

$$P = 2{\cdot}5505 \times {\cdot}0252 = {\cdot}064.$$

* *Méc. Cél.* VIII. xvii. § 36.

† It may be observed, that to be thoroughly consistent we should have in this case, $P = {\cdot}663m = {\cdot}873\epsilon$, because $\frac{5m}{2\epsilon} = 3{\cdot}290$; but the method is here only used to find the limiting value of the coefficient of $\epsilon - \frac{m}{2}$.

I conceive then, that *Saturn's* precessional constant must be very nearly equal to ·06, since the former of these values is certainly too small, whilst the latter is a little too large.

5. *Mars.*

Mars rotates in $24^h\ 37^m\ 22^s{\cdot}6$, or 1·025956 m. s. days, according to the elaborate observations of M. Kaiser.* His density is ·948 of that of the Earth.† The sidereal day is ·997270 m. s. days, and for the Earth m is $\frac{1}{289{\cdot}66}$; hence for *Mars*,

$$m = \frac{1}{{\cdot}948}\left(\frac{{\cdot}997270}{1{\cdot}025956}\right)^2 \frac{1}{289{\cdot}66}$$

$$\fallingdotseq {\cdot}0034409 = \frac{1}{290{\cdot}6}.‡$$

With respect to the ellipticity of the planet the most various estimates are given. Thus, for the reciprocal of the ellipticity I find, according to Loomis (*Pract. Astr.*, Table 33) 50; Main (*Month. Not. Ast. Soc.* xvi., p. 142) 62; Kaiser (Guillemin, *Le Ciel*, p. 257) 118; Arago, doubtfully, 32; Bessel failed to detect ellipticity in observations at Königsberg; and lastly, from observations of Dr. Winnecke, to which I shall recur below, 225 (*Ast. Nach.* 48, p. 97). According to Dr. Winnecke, M. Kaiser doubts whether Mr. Main's result represents any corresponding reality.

With an ellipticity of $\frac{1}{225}$, we have $\frac{5m}{2\varepsilon} = 1{\cdot}93$, and with the other values of ellipticity, a very much smaller value.

Now a reference to the table in section 1 will show that not only is even the largest of these quantities incompatible with the Laplacian law of density, but they are also incompatible with the homogeneity of the planet. We should require the planet to decrease in density towards the centre, or actually to be hollow; and on any theory of original fluidity such a state of things is almost inconceivable.

The wide discrepancy between various observations shows that the results are to be very little depended on; and for the following reasons, I venture to think that all the above values of the ellipticity are almost worthless, but that Dr. Winnecke's is by

* See a paper by M. Julius Schmidt, *Ast. Nach.* 82, p. 333.

† Guillemin, *Le Ciel*, p. 257. The other details with respect to *Mars* are given on the authority of M. Kaiser, and this I presume is so also.

‡ Plana gives $\frac{1}{288{\cdot}84}$, using slightly different data; with Herschel's value of the density ·72, $m = \frac{1}{220{\cdot}7}$.

far the nearest to the truth. He gives* as the equatoreal and polar apparent diameters, 9″·227 and 9″·186, subject to mean errors of ·045″ and ·032″ respectively.

From these measurements I find that the *probable error* of the ratio of the axes is ·0050441. The ratio itself is in decimals ·9955564, and therefore the ratio of the axes is ·9955564 ±·0050441. That is to say, it is an even chance that the ratio of the axes lies between the 1·0006 and ·990512. In other words, it results from Dr. Winnecke's observations that it is an even chance that the figure of *Mars* lies between a prolate spheroid, with an ellipticity of $\frac{1}{1667}$, and an oblate one with an ellipticity of $\frac{1}{105}$; the observed values give an ellipticity of $\frac{1}{225}$.

Dr. Winnecke himself says, "Es sprechen diese Messungen entschieden gegen eine Abplattung des *Mars*, die für das Berliner Instrument messbar wäre."

Now the telescope shows that *Mars* has an external physical constitution exceedingly like that of the Earth, and therefore there is a strong probability that its internal structure is somewhat the same. Seeing then that the observations of the best observers permit such very wide limits of error, and that the value of m can be assigned with some precision, I submit that there is a far better chance of being near the truth in trusting to indirect evidence than to direct observation, for the determination of the ellipticity. Assuming then that the law of internal density is the same as in the Earth, I find an ellipticity of $\frac{1}{298}$, and this, I venture to think, is nearer the truth than any of the above-quoted values derived from observation.

6. *Mercury, Venus.*

M. Plana assigns as the values of m for these planets, $\frac{1}{325{\cdot}82}$ and $\frac{1}{251{\cdot}54}$ respectively; and following the arguments used in the case of *Mars* (although they have not here equal force), we should assign to *Mercury* an ellipticity of about $\frac{1}{330}$, and to *Venus* of about $\frac{1}{260}$.

7. *Conclusion.*

The results arrived at may be summed up as follow:—

The values assigned by Laplace to the precessional constants of *Jupiter* and *Saturn* cannot be maintained; and the objections

* *Ast. Nach.* 48, p. 97. The observations are said to be reduced for "Phase und Refraction," but I do not see it expressly stated that they are corrected for the Earth's Martian declination.

to them remain equally strong when later observations are consulted.

Professor Adams's calculation of $\epsilon - \frac{m}{2}$ gives, as the ellipticity of *Jupiter*, $\frac{1}{16{\cdot}02}$.

The surface density of *Jupiter* is far less than the mean, and he may perhaps still be in a semi-nebulous condition.

There is a considerable probability that the like is true of *Saturn*.

The value of *Jupiter's* precessional constant is about ·0548; and that of *Saturn* probably about ·06. No dependence is to be placed on the observations of the ellipticity of *Mars*, because of their wide limits of error. Indirect evidence of the ellipticity seems safer, and we are thereby led to assign to him an ellipticity of $\frac{1}{298}$. In the cases of *Mercury* and *Venus*, we have only indirect evidence to rely on, and these ellipticities are probably about $\frac{1}{330}$ and $\frac{1}{260}$.

In conclusion, I must add that if in any case I have underrated or neglected the work of important observers, or have overrated the worth of other observations, my excuse must be my previous slight acquaintance with observational astronomy.

Postscript.—The method of this paper may perhaps give some idea of the amount of difference which might be expected to be found between the apparent equatoreal and polar diameters of the Sun.

To find m, the Earth may be treated as a solar satellite, and the same formula as before applied. $\frac{d}{D}$ is here the Sun's mean apparent radius (which I take as 961″·82) in circular measure.

T = 365·2569 m. s. days.

t, according to Carrington, is 25·38 m. s. days.

But Mr. Christie (to whom I owe my thanks for his help in this matter) informs me that the true period may perhaps be 26 or 27 days. The following results are therefore given in duplicate; in those marked with an accent t is taken as 27.

Then $m = {\cdot}00002100$, or $m' = {\cdot}00001829$.

If the law of internal density were the same as in the Earth, we should have,

$\epsilon = {\cdot}00002048$, or $\epsilon' = {\cdot}00001784$.

These correspond to differences of apparent diameter of ·0394″ and ·0343″ respectively. If the Laplacian law of density were to hold good, and if the surface density were zero, we should have, $\epsilon = {\cdot}00001596$, or $\epsilon' = {\cdot}00001390$, which correspond to differences of ·0307″ and ·0267″ respectively.

Mr. Christie informs me that the probable error in the de-

termination of the Sun's apparent diameter is not less than 0·1″, and it appears that the observed difference of diameters is also 0·1″.

May we not conclude that the difference of the apparent diameters is probably less than ·04″, but perhaps greater than ·03″ ?

Note on the Relative Space-penetrating Power of the Pulkowa 15-inch Refractor and Mr. Lassell's 4-foot Reflector. By M. Otto Struve.

In his paper "On the Relative Power of Achromatic and Reflecting Telescopes" (*Monthly Notices*, vol. xxxvi. pp. 305–309) Dr. Robinson makes the following statement:—

"2. Otto Struve *thought* that the Pulkowa Achromatic of 15-inches was equal to Lassell's 24-inch Newtonian (not his 4-feet, as supposed by M. Radau): nothing definite can be built on this."

I feel sure, if Dr. Robinson had read my paper, "Ueber das von Herrn W. Lassell in Malta aufgestellte Spiegelteleskop," published in the beginning of 1864 in the *Bulletin de l'Académie de St.-Pétersbourg*, he would have considerably modified his words, and perhaps also his views. In fact, M. Radau has been quite correct in his statement. I wrote the above-mentioned paper immediately after having visited my honoured friend Mr. Lassell at Malta. Together with some other results derived from practical experience, I have given there, *in extenso*, the comparison of the ultima visibilia in the nebula of *Orion*, observed at Pulkowa by Dr. Winnecke, simultaneously with me, at Malta. The result of this comparison is expressed in the following terms:—"The preceding comparison of almost simultaneous observations made at Malta and Pulkowa proves evidently that the space-penetrating power of Lassell's new (4-feet) instrument can hardly be estimated superior to that of ours (the Pulkowa 15-inch) refractor; especially if we consider that at Pulkowa the nebula of *Orion* has been observed at a height of only 25°, while at Malta the observations were made at the height of 49°."

I beg leave to add that in 1863, as a great many comparisons made about that time have proved, my eye was still quite of the same strength with that of Dr. Winnecke.

Pulkowa, 1876, November.

On the Position of the Point of Maximum Brightness on Venus. By E. Neison, Esq.

Supposing *Venus* to reflect the solar rays from a dead or unpolished surface, the intensity of illumination of the surface at any point P will be proportional to the cosine of the zenith

distance of the Sun (=z) at that point. Then from a known optical principle, the apparent brightness of the surface at P as seen from the Earth will be proportional to the real illumination. Consequently, the apparent brightness B of any point on *Venus* as seen from the Earth will be

$$B = A \cos z.$$

The point where the apparent brightness is greatest will be therefore that where the Sun is in the zenith.

Consequently, when *Venus* is crescent, or less than one half of the illuminated hemisphere of *Venus* is visible, the limb of the planet will be the brightest portion. This is what the observations of Capt. Noble (*Monthly Notices*, vol. xxxv. p. 350) have already indicated. When, however, the planet is gibbous, so that more than half the illuminated hemisphere can be seen, then the point of maximum illumination will be within the limb. Under these conditions, in fact, the region of maximum brightness will be an ellipse of great eccentricity.

This is in accord with the observations of Mr. Christie, communicated to the Society in November 1876.

These two series of observations are not therefore, discordant, as might be supposed, and neither confirm the hypothesis that the solar rays are specularly reflected from the surface of *Venus*.

Note on the Gradation of Light on the Disk of Venus.

By W. H. M. Christie, Esq.

Some months ago Mr. Brett pointed out that there was a gradation of brightness towards the limb of *Venus* as well as, in a more marked degree, towards the terminator, and he considered that this was due to specular reflexion modified by large atmospheric diffusion. As *Venus* was then becoming crescent-shaped, and therefore unfavourably situated for showing any effect of specular reflexion, I had no opportunity of testing the point till after inferior conjunction. When the planet became gibbous again, I examined the disk on several occasions in the forenoon with the direct-vision polarising eyepiece, described in the *Monthly Notices* for last January, applied to the Great Equatoreal of the Greenwich Observatory, and was thus enabled to reduce the brightness very gradually to the point of total disappearance. On October 12 the disk was reduced, on turning the Nicol prism, to an oblong patch, the edge of which was distant about one-sixth of the diameter of *Venus* from the limb, special attention being directed to this point. Afterwards, on November 28 and December 6, under more favourable circumstances, four distinct phases were remarked, corresponding to different readings of the position-circle attached to the Nicol, and indicating four different degrees of brightness:

(*a*) At the first phase the limb was just beginning to fade, the disk being then gibbous, though a small portion near the terminator had, as far as I could judge, already disappeared.

(*b*) The next phase was an oblong patch, from two-thirds to three-quarters of the planet's diameter in length, in the direction of the line of cusps, and about one-third as broad as it was long.

(*c*) On turning the Nicol further, the ends of this patch disappeared, and a nebulous spot, about one-third of the diameter of *Venus*, and condensed towards the centre, was seen.

(*d*) Finally, just before total disappearance, the disk was reduced to a nearly stellar point from one-sixth to one-tenth of the original diameter, and surrounded by an extremely faint haze, which was occasionally invisible, leaving nothing but a minute point of light to be seen. The appearance was exactly that of the nucleus of a small comet, or of a nebulous star. By the help of a pointer in the field of view, the position of this stellar point was found to be about the middle of the visible disk.

From several sets of closely accordant readings of the position-circle, taken with the eyepiece in various positions, the angles for the Nicol measured from the position for which its plane of polarisation is perpendicular, to the plane of reflexion in the eye-piece, were found as follows for the respective phases :—

(*a*) 30° (*b*) 24° (*c*) 13° (*d*) 11°

Since the intensities of light transmitted by the Nicol are as the squares of the sines of the above angles, we get, taking the faintest portion of phase *a* as standard:—brightness of $b=1{\cdot}5$, of $c=4{\cdot}8$, of $d=7{\cdot}0$. So that it would appear that the central portion of the disk is about seven times as bright as the part near the limb. It may be thought strange that so great a difference in brightness should not be more noticeable, but it would seem that a highly-trained eye is required to detect a gradation in bright objects, though when the brightness is reduced nearly to the vanishing point, there is no difficulty in seeing it. In the polarising eyepiece the light is reduced in the first instance to about 1–3000th part by the three reflexions, and with the Nicol at the reading for phase *d* it is further reduced to one-thirtieth of this amount, or 1–90,000th of the original brightness. Though the extinction method of photometry is liable to large error from variation in the sensitiveness of the eye, it may be trusted for comparative results, where, as in this case, there is an extremely faint background of scattered daylight, which gives a sort of standard for comparison.

Whatever uncertainty there may be in the actual values found for the brightness, there can, I think, be no doubt of the fact of a considerable gradation of light from a central, or nearly central spot; and it seems difficult to explain this on any other hypothesis than that of specular reflexion modified by atmospheric diffusion.

1876, *December* 7.

On Spheroidal Trigonometry. By Prof. Cayley.

The fundamental formulæ of Spheroidal Trigonometry are those which belong to a right-angled triangle PSS_o, where P is the pole, PS, PS_o arcs of meridian, and SS_o a geodesic line cutting the meridian PS at a given angle, and the meridian PS_o at right angles. We consider a spherical triangle PSS_o,

Sides	PS,	PS_o,	$SS_o = \gamma$,	γ_o,	s,
Angles	S_o,	S,	P $= 90°$,	θ,	l,

where γ is the reduced colatitude of the point S on the spheroid (and thence also γ_o the reduced colatitude of S_o) and θ the azimuth of the geodesic SS_o, or angle at which this cuts the meridian SP; and then if S be the length of the geodesic SS_o measured as a circular arc, radius = Earth's equatoreal radius, and L be the angle SPS_o, S, L differ from the corresponding spherical quantities s, l by terms involving the excentricity of the spheroid, viz. calling this e and writing

$$k = \frac{e \cos \gamma_o}{\sqrt{1 - e^2 \sin^2 \gamma_o}},$$

then (see Hansen's *Geodätische Untersuchungen*, *Abh. der K. Sächs. Gesell.*, t. viii. (1865) pp. 15 and 23, but using the foregoing notation) we have, to terms of the sixth order in e,

$$\frac{S}{\sqrt{1-e^2}} = \left(1 + \frac{1}{4}k^2 + \frac{13}{64}k^4 + \frac{45}{256}k^6\right)s + \left(\frac{1}{8}k^2 + \frac{3}{32}k^4 + \frac{79}{1024}k^6\right)\sin 2s + \left(\frac{1}{256}k^4 + \frac{5}{1024}k^6\right)\sin 4s + \frac{1}{3072}k^6 \sin 6s;$$

and

$$L = l - \frac{1}{2}e^2 \sin\gamma_o \left\{\left(1 - \frac{1}{8}k^2 + \frac{1}{4}e^2 - \frac{5}{64}k^4 + \frac{1}{8}e^4\right)s - \left(\frac{1}{16}k^2 + \frac{1}{32}k^4\right)\sin 2s + \frac{1}{256}k^4 \sin 4s\right\},$$

which are the formulæ in question.